Quantum Times: Physics, Philosophy, and Time in the Postwar United States

Quantum Times: Physics, Philosophy, and Time in the Postwar United States

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Abstract

The concept of time in physics underwent significant changes in the decades following World War II. This dissertation considers several ways in which American physicists grappled with these changes, analyzing the extent to which philosophical methods and questions played a role in physicists' engagement with time. Two lines of questioning run through the dissertation. The first asks about the professional identities of postwar American physicists in relation to philosophy, as exemplified by their engagement with the concept of time. The second analyzes the heterogeneous nature of time in physics, and the range of presuppositions and assumptions that have constituted this "fundamental" physical concept.

The first chapter looks to the development of atomic clocks and atomic time standards from 1948-1958, and the ways in which new timekeeping technologies placed concepts such as "clock", "second," and "measure of time" in a state of flux. The second chapter looks to the experimental discovery of CP violation by particle physicists in the early 1960s, raising questions about nature of time understood as the variable *"t"* in the equations of quantum mechanics. The third chapter considers attempts to unify quantum mechanics and general relativity in the late 1960s, which prompted physicists to question

the “existence” of time in relation to the universe as a whole. In each episode considered, physicists engaged with the concept of time in a variety of ways, revealing a multiplicity of relationships between physics, philosophy, and time. Further, in each case physicists brought a unique set of assumptions to their concepts of time, revealing the variety ways in which fundamental concepts functioned and changed in late twentieth century physics. The result is a heterogeneous picture of the practice of physics, as well as one of physics’ most basic concepts.

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INTRODUCTION

1.

The concept of time is a central one for contemporary scholars in a range of disciplines, including physics, philosophy, psychology, anthropology, and literary theory. As such, it is a particularly appropriate point of entry for investigating relationships among the disciplines of the late twentieth century. What do conceptions of time, as employed by different groups of scholars, have in in common and how do they differ? How are questions about time framed differently in various disciplines, and which methods are used to answer these questions? What presuppositions about the nature and boundaries of a given discipline contribute to its practitioners' treatments of time? Even within a traditional academic discipline such as physics, there are subgroups of researchers who understand the concept of time, and the intellectual problems it poses, very differently. Thus, a study of the ways scholars have handled the concept of time can illustrate the relations among disciplines, as well as within a given discipline.

This dissertation will examine how different subgroups of physicists, working in the United Sates in the 1950s and 1960s, approached fundamental questions about time. Specifically, it will consider the extent to which each subgroup asked questions about the meaning and nature of time, as well as how speculative and interpretive methods factored into their answers. Within each subgroup, physicists operated under a particular understanding of what it meant to do physics, influencing the types of questions they asked and how freely they speculated about the nature of their central concepts. The primary objective of this dissertation will be to account for the various ways American physicists understood their work and discipline in the decades following World War II, as exemplified by their engagement with the concept of time. This will set the stage for a secondary objective: to understand what a fundamental concept, like time, looks like when situated within shifting understandings of physics.

Each chapter focuses on a specific episode in the history of physics in the postwar United States in which the nature of time emerged as a salient issue. The first chapter considers a subgroup of physicists working on the development of atomic clocks and atomic time standards in the 1950s, who engaged in debates over the universality of *measures* of time. The second chapter considers a subgroup of experimental particle physicists in the early-1960s, whose work raised questions about the *directionality* of time. The third chapter looks to a subgroup of physicists seeking to unify quantum mechanics and general relativity in the late-1960s, who argued about the *existence* of time. In each case, leading physicists approached questions about time in remarkably different ways, revealing a variety of relationships among interpretation, physics, and time. The subgroup working on atomic clocks and standards in the 1950s did not overtly acknowledge that their work raised questions about the nature of time, although their definitions of basic timekeeping concepts were in flux. The subgroup of experimental particle physicists in the early 1960s identified questions about the nature of time as at stake in their research, but did not pursue these questions themselves, considering them to be outside the boundaries of their discipline. Finally, the subgroup working on the unification of quantum mechanics and general relativity understood interpretive questions and methods as central to their work.

I use the word "subgroup" to refer to clusters of physicists, working directly alongside one another, in pursuit of a related set of problems. Rather than examining "physicists" at the general level, or the large and diverse groups designated by the labels "theorists" and "experimentalists," I take small groups of physicists, directed toward a specific objective, as my unit of analysis. These subgroups were narrower in scope than professionally designated fields of physics, such as the divisions named by the American Philosophical Society (APS), and did not necessarily persist as cohesive units throughout the postwar period. Rather, during the episodes at stake in this dissertation, each subgroup coalesced around a common goal: developing atomic clocks and time standards in chapter one, describing symmetry relations in particle interactions in chapter two, and uniting quantum mechanics and general relativity in chapter three. This being said, each subgroup approached their work from within the context of a field of research that corresponds closely to the scale of an APS division. The atomic timekeeping subgroup belonged largely to the field of molecular beam research, the subgroup working on symmetries belonged to the field of particle physics, and the quantum gravity subgroup operated from within the context of general relativity research. Throughout the dissertation, I use the attitudes of members of the specific subgroups in question to gain insight into the sensibilities of the larger fields of research in which they were embedded, and vice versa. I then set these insights in relation to one another in order to draw more general conclusions about the overall landscape of professional attitudes among postwar

American physics.

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This dissertation builds on a body of literature that explores a tension in twentieth century physics between a philosophical approach to physics that was a feature of European physics in the first half of the twentieth century, and a more pragmatic approach that characterized much of American physics following World War II.[1] For example, in his 2011 book *How the Hippies Saved Physics*, David Kaiser describes how famous European physicists of the 1920s and 30s, such as Niels Bohr and Albert Einstein, believed "that progress could only be made by tackling […] philosophical challenges head on. Manipulating equations for their own sake would never be enough."[2] Kaiser contrasts this position with the attitudes of postwar American physicists, who believed "their business was to calculate, not to daydream about philosophical chestnuts."[3] Kaiser

[1] See for example Silvan S. Schweber, "The Empiricist Temper Regnant: Theoretical Physics in the United States, 1920—1950," *Historical Studies in the Physical and Biological Sciences* 17 (1986): 55-98; Alexi Assmus, "The Americanization of Molecular Physics," *Historical Studies in the Physical and Biological Sciences* 23, no. 1 (1992):1 – 34; Nancy Cartwright, "Philosophical Problems of Quantum Theory: The Response of American Physicists", in *The Probabilistic Revolution, V2*, ed. Lorenz Kruger et al. (Cambridge, MA: MIT Press, 1987); Peter Galison, *How Experiments End* (Chicago: University of Chicago Press, 1987); Peter Galison, *Image and Logic* (Chicago: University of Chicago Press, 1997); and David Kaiser, *How the Hippies Saved Physics: Science, Counterculture, and the Quantum Revival* (New York: W.W. Norton and Company, 2011).

[2] Kaiser, *How the Hippies Saved Physics*, xiii.

[3] *Ibid.,* xiv.

thus presents two contrasting pictures of what it meant to be a physicist in the twentieth century – one in which interpretive, speculative thinking was central to the project of physics, and another in which the purpose of physics research was obtaining useful results.

Scholars have offered various accounts of when and how the more pragmatic sensibility came to characterize American physics. For example, several historians of science have located the emergence of a pragmatic sensibility among American physicists in the first half of the twentieth century. In his 1986 paper "The Empiricist Temper Regnant," Sam Schweber attributes the pragmatic mindset among American theoretical physicists to the close contact between theoreticians and experimentalists in American research universities following World War I,[4] as well as the influence of American pragmatist thinkers such as Peirce, Dewey, and Bridgeman.[5] Along similar lines, in her 1989 paper "Philosophical Problems of Quantum Theory," Nancy Cartwright describes how pre-World War II American physicists were influenced by the "philosophy of science that most of them shared, a philosophy akin, but not identical, to the well-known American doctrines of pragmatism and operationalism."[6] Other scholars have pointed to the emergence of a pragmatic sensibility in the wartime years, in the context of the contact between physics and engineers, as well as the practical, technical problems with which physicists were faced. For example, David Kaiser describes how a form of

[4] Schweber, "The Empiricist Temper Regnant," 54.

[5] *Ibid.,* 61-65.

[6] Cartwright, "Philosophical Problems of Quantum Theory," 417.

pragmatism became entrenched in the culture of American physics following World War II:

> Torn from their prewar routines and thrust into projects of immediate, worldly significance – radar, the atomic bomb, and dozens of lesser-known gadgets – physicists' day-to-day activities in 1945 bore little resemblance to those of 1925. […] Physicists in the United States adopted an aggressively pragmatic attitude. The equations of quantum mechanics had long since lost their novelty, even if their ultimate meaning still remained obscure. The pressing challenge became to put those equations to work.[7]

Peter Galison has also identified World War II as pivotal in the emergence of an increasingly pragmatic mindset among physicists, arguing that physicists' direct encounters with machine culture and instrument making during wartime affected the ways in which they approached physics after the war.[8]

In this dissertation I will not be explicitly concerned with the questions of when and how American physicists came to be more pragmatic about their work, although I will refer to continuities and discontinuities between prewar, wartime, and postwar attitudes, methods, and tools as they become relevant. Rather, the tension between pragmatic and philosophical sensibilities among postwar American physicists will serve as the point of departure for my analysis. My objective will be to unpack the ways in

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[7] Kaiser, *How the Hippies Saved Physics*, xiii.

[8] See for example Peter Galison, "Feynman's War: Modelling Weapons, Modelling Nature," in *Studies in the History and Philosophy of Modern Physics* 29 (1998): 391-434; Peter Galison, "Structure of Crystal, Bucket of Dust," in *Circles Disturbed, the Interplay of Mathematics and Narrative*, eds. Apostolos K. Doxiadēs and Barry Mazur (Princeton: Princeton University Press, 2012), 52-78; and Peter Galison, *Image and Logic: A Material Culture of Microphysics* (Chicago: Chicago University Press, 1997), Ch. 4.

which subgroups of physicists forged out their professional identities in relation to the competing pulls of philosophy and pragmatism. I will consider how, why, and to what extent postwar American physicists engaged in a particular form of philosophical questioning in physics; that is, investigation into the deeper meaning of their basic concepts. Throughout, I will use the term "philosophy" to refer to this type of interpretive thinking, while recognizing it represents one of many ways one could pose philosophical questions about physics and time.[9] This being said, other forms of philosophical questions will enter the conversation as they arise in physicists' discourse, for example surrounding ethics, consciousness, and the meaning of life. Further, I will loosely use "pragmatism" to refer to the valuation of useful results over deep understanding among physicists, while recognizing that pragmatism also took many forms during this period. By focusing on moments when subgroups of physicists encountered basic questions about the meaning of time, and examining how they engaged or failed to engage these questions, a complex and heterogeneous picture of the identities of postwar American physicists, in relation to the competing pulls of philosophy and pragmatism, will emerge.

The scope of the dissertation is limited to subgroups of American physicists, although physicists from the international community will be discussed insofar as they are relevant to the American subgroups in question. The United States serves as the geographical focus due to the unique ways in which the tension between philosophical

[9] For a discussion of how this type of investigation became the prevailing mode of philosophical questioning about time in physics, see Jimena Canales, "Einstein, Bergson, and the Experiment that Failed: Intellectual Cooperation at the League of Nations," *MLN* 120 (2005): 1168–1191.

and pragmatic sensibilities played out in postwar America. Each chapter will consider one small section of the larger landscape of the professional identities of postwar American physicists; while doing so, a second line of questioning will arise regarding the concept of time itself. How did physicists' understandings of time change during this period? Further, what can be said about the concept of time in general, after it has been examined in several contexts and on multiple registers? This will involve analyzing the many contours of physicists' assumptions about time, showing the plurality and contingency of these assumptions. Each chapter will consider a different set of assumptions about time and the degree to which physicists questioned these assumptions. The result will be a heterogeneous picture of one of physics' most basic concepts. The conclusion will take this line of thinking one step further, using the insights gained from each chapter to analyze physicists' assumptions about "fundamental concepts" and "conceptualization" in general.

2.

Each chapter considers a specific community of postwar American physicists, as well as a specific set of questions about time. In doing so, each draws upon studies in the history and philosophy of science that have analyzed these communities and questions. For example, chapter one builds on literature surrounding standardization and metrology in the history and philosophy of science, including work by Ken Alder, Peter Galison, Hasok Chang, Simon Schaffer, and Robert Crease.[10] Chapter two makes use of scholarship about the culture and categories of high-energy physics in the postwar period, including the work of Andrew Pickering, Peter Galison, and David Kaiser.[11] Chapter three engages scholarship about the community of general relativity researchers in the twentieth century, including work by David Kaiser, Jean Eisenstaedt, Daniel Kennefick, and Dean Rickles.[12] Sections 2.1, 2.2, and 2.3 below will briefly describe the stakes of each chapter as they stand in relation to the bodies of scholarly literature on which they build. Looking more specifically at the arguments of each chapter and the historiographical context for these arguments, these sections will delineate the two lines

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[10] Ken Alder, "A Revolution to Measure: The Political Economy of the Metric System in France" in *The Values of Precision* (Princeton: Princeton University Press, 1995), 39-71; Peter Galison, *Einstein's Clocks and Poincare's Maps: Empires of Time* (New York: W.W. Norton & Company, 2003); Hasok Chang, *Inventing Temperature*, (Oxford: Oxford University Press, 2004); Simon Schaffer, "Late Victorian Metrology and Its Instrumentation: A Manufactory of Ohms," in *the Science Studies Reader*, ed. Mario Biagioli (New York: Routledge, 1999), 457-478; Robert Crease, *World in the Balance: The Historic Quest for an Absolute System of Measurement,* (New York: W.W. Norton & Company, 2011).

[11] Andrew Pickering, *Constructing Quarks: A Sociological History of Particle Physics* (Chicago: University of Chicago Press, 1984); Peter Galison, *How Experiments End* (Chicago: University of Chicago Press, 1987); Peter Galison, *Image and Logic: A Material Culture of Microphysics* (Chicago, Chicago University Press, 1997); David Kaiser, *Drawing Theories Apart: The Dispersion of Feynman Diagrams in Postwar Physics* (Chicago: University of Chicago Press, 2005).

[12] Jean Eisenstaedt, *The Curious History of Relativity: How Einstein's Theory of Gravity was Lost and Found Again* (Princeton: Princeton University Press, 2006); David Kaiser, "A Psi is just a Psi? Pedagogy, Practice, and the Reconstitution of General Relativity, 1942-1975," *HSPS* **33** (2002): 131-159; Daniel Kennefick, *Travelling at the Speed of Thought: Einstein and the Quest for Gravitational Waves* (Princeton: Princeton University Press, 2007); Dean Rickles, "Quantum Gravity: A Primer for Philosophers," in *The Ashgate Companion to Contemporary Philosophy of Physics*, ed. Dean Rickles, 2008.

of questioning that run through the dissertation - respectively concerning the professional identities of postwar American physicists and the nature of time as one of their fundamental concepts – more concretely.

2.1: Atomic Clocks and Standards

Chapter one describes the development of the first atomic clocks and time standards in the 1950s. Specifically, it treats the period beginning in 1948, when the first "clock" based on the quantum properties of matter was built, up until 1958, when the first atomic time standard was established. In chapter one I argue that during this period timekeeping concepts were in a state of flux, with physicists and astronomers renegotiating the meaning of concepts such as "clock," "second," and "time" in light of new atomic clock technology. I trace the factors that contributed to this period of conceptual renegotiation, including the institutional and disciplinary allegiances of the physicists involved, their values with respect to time-keeping, and the specific features of the new instruments and techniques with which they were working. I show that despite the changes that occurred in the way this subgroup of physicists understood basic time-keeping concepts, they viewed philosophical questions about the meaning of time to be firmly outside the domain of physics. Although philosophical questions were at stake in their work, they did not explicitly discuss them as such.

Units of measure are arguably among the most foundational concepts in physics; thus, a change in the definition a unit of measure provides an excellent point of entry for unpacking physicists' basic assumptions about their fundamental concepts. Several historians of science have studied changes in standard units of measure to gain insight into how basic scientific concepts are created, adopted, and disseminated.[13] These studies show how metrological standards, which are often assumed to be objective and universally valid, are in fact the products of contingent value systems. Ken Alder argues along these lines in his article "A Revolution to Measure: The Political Economy of the Metric System in France," claiming that "at the core of 'universal standards' commonly taken to be the product of objective science lies the historically contingent."[14] Alder goes on to describe how "these seemingly 'natural' standards express the specific, if paradoxical, agendas of specific social and economic interests."[15] Along similar lines, Hasok Change argues in *Inventing Temperature* that units of measure, the "simple items of knowledge that we take for granted," are in fact "spectacular achievements, obtained only after a great deal of innovative thinking, painstaking experiments, bold conjectures, and serious controversies which may in fact never have been resolved quite satisfactorily."[16]

What is the process by which old standards are replaced with new ones? Several scholars have noted the seemingly circular way in which standard units of measure come to be redefined. Robert Crease has described the redefinition of standard units of measure as a "bootstrapping" process, in which a property that has been calibrated to a standard

[13] See note 9.

[14] Alder, "A Revolution to Measure," 39.

[15] *Ibid.*

[16] Chang, *Inventing Temperature*, 4.

becomes the standard, in turn becoming unmeasurable.[17] Hasok Chang's *Inventing Temperature* analyzes the complexities of this bootstrapping process with respect to the study of heat in the 18th and 19th centuries. Peter Galison's *Einstein's Clocks and Poincare's Maps* further unpacks this process in relation to the redefinition of international metric standards in the late 19th century. Chapter one of this dissertation builds upon the work of Chang and Galison, looking to the process by which time standards were redefined in the mid-twentieth century.

During the period considered in chapter one, physicists developed an atomic definition of the second that supplanted previous definitions based on astronomical observations. This change was part of a larger trend toward atomic definitions of units of measure, which by the late twentieth century obtained for all standard international measures except for the kilogram.[18] As opposed to definitions based on the specifications of physical objects, such as the platinum bars that have been used to define the meter, standards came to be defined by quantum processes within atoms, specifically the wavelengths and frequencies of radiation emitted during atomic transitions. These standards represented a new ideal of universality, as in theory they were reproducible anywhere in the universe. As Robert Crease has described this ideal: "For the first time in history, if all basic standards were somehow lost, they could be recovered and the world would have exactly the same measurement standards as before."[19] Nevertheless, the

[17] Crease, *World in the Balance*, 252.

[18] See Crease, *World in the Balance,* 15.

[19] *Ibid.*

redefinition of standards in terms of atomic processes were embedded in historical contingencies, and involved unique and complicated forms of “bootstrapping”.

Robert Crease’s *World in the Balance* begins to describe the historical transition toward atomic standards, beginning in the 19th century. Crease tells the story primarily with respect to the establishment of new standards for units of length, specifically the redefinition of the meter in terms of wavelengths of light. He begins with the work of C.S. Peirce in the mid nineteenth century, who according to Crease was the first person to attempt to define the meter in terms of wavelengths of light, with an experimental apparatus involving diffraction gratings.[20] As Crease describes, Peirce’s instruments lacked the precision required to arrive at a meaningful result. In the meantime, Albert Michelson took up a similar project upon learning of Peirce’s work, and in 1887 published the results of his efforts using his interferometer.[21] Michelson’s publication began a long process that led to a 1952 decision by the Bureau International de Poids et Mesures (BIPM) to officially redefine the meter in terms a wavelength of chosen frequency, effective beginning in 1960.[22] As Crease describes: “After the redefinition […] would take place, the wavelength of the chosen spectral line would no longer be measurable; it would be the ruler, not the ruled.”[23] The chosen frequency was a line in the spectrum of the Krypton-86 atom, which served as the foundation of the standard meter

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[20] *Ibid*., 195-202.

[21] In 1907 Michelson won the Nobel Prize, largely for this work.

[22] Crease, *World in the Balance*, 215.

[23] *Ibid.,* 215.

until 1983, when the meter was again redefined, this time in terms of the speed of light and the then-established atomic second.[24]

Crease discusses the establishment of the atomic second as similar in kind to the redefinition of length in terms of spectral wavelengths, without elaborating any of the specific details. However, as chapter one of this dissertation shows, the redefinition of the second in terms of atomic processes was embedded in a unique set of contingencies and values that distinguish it from the redefinition of the meter. Further, the process by which the atomic second was established raised a unique set of conceptual problems about timekeeping. Before the development of atomic time, the second was defined in terms of astronomical observations tied to the cycles of day and night, as well as the change of seasons as experienced on earth. By redefining the second in terms of atomic processes, measures of time became officially divorced from such cycles of experience. Thus, the redefinition of the second in atomic terms raised questions about what a "measure of time" *is*, as well as about the connection between time as measured in the lab and time as a feature of everyday experiences of change. While some physicists aspired to the perceived "universality" of an atomic definition of the second, others felt that such a definition inappropriately divorced time from human experience. Further, the technical establishment of the atomic second involved novel processes, particularly a series of feedback loops, resonance effects, and fine tunings. This technique raised the question of where the standard second is "located" within the complicated technical processes that now serve as atomic time standards. Chapter one explores the specific issues that

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[24] *Ibid.*

accompanied the atomic definition of the second, including the values at stake and the new technical processes involved, showing it to be a unique and revealing moment in the history of standardization and metrology.

Overall, the first chapter looks to what is often taken for granted as objective and universal – the atomic definition of the second - and unpacks the contingencies and conceptual issues out of which it emerged. The second, as defined by atomic processes, is foundational in twenty-first century physics; however, in the 1950s the concept of the second, along with other timekeeping concepts, was in a state of flux. Yet arguably more foundational than the concept of the "second" is the concept of "time" itself. What can a study of the redefinition of the second reveal about the assumptions about that which the second is a measure *of*? Is there a background concept of time that physicists presuppose as "true", independent of specific time standards? This dissertation will make it clear that there is no single truth of time; time per se is as conventional and heterogeneous as the units used to measure it. However, the conclusion to this dissertation will further argue that physicists must necessarily presuppose that there is a "truth" about time before they can begin to discuss and use the concept. Chapter one will begin to address this issue by looking at physicists' presuppositions about time in the development of atomic clocks and time standards, revealing a plurality of presuppositions and assumptions about the concept, as well as a common presupposition about the commensurability of times understood in different contexts. Set in relation to concepts of time presupposed by the physicists considered in chapters two and three, this will contribute to the deeply heterogeneous picture of one of physics' most basic concepts, as well as an account of what holds this concept together.

2.2 Chapter Two: Particle Physics and the Direction of Time

Chapter two considers the work of experimental particle physicists whose research affected notions about time as a physical variable at the intersection of physical laws and experimental practice. In particular, it looks to a development in experimental particle physics in the 1960s that diverged from the consensus understanding among physicists about the directionality of time in physical laws. The directionality of time can been regarded as one of time's defining characteristics, as well as the characteristic that differentiates it from space;[25] thus, insight into the directionality of time could potentially provide insight into the nature of time per se. Experimental particle physicists Val Fitch and James Cronin encountered this potential in the early 1960s, when they ran an experiment that implied the fundamental laws of physics are asymmetrical with respect to the direction of time. This result touched upon a longstanding line of questioning about whether or not the laws of physics take different forms in the forward and backward directions of time, and went against the accepted conclusions of this line of questioning. Cronin and Fitch acknowledged that their work impacted philosophical and physical questions about the directionality of time; however, they did not engage such questions. While they exhibited certain philosophical tendencies in their approach to physics, at the

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[25] It is described as such, for example, in Hans Reichenbach, *The Direction of Time* (Mineola, New York: Dover Publications, Inc., 1956).

level of their explanations of the impact of their experiment, their understanding of the practice of physics did not leave room for discussion of philosophical implications of their results for the meaning of a basic concept like time.

From the point of view of everyday human experience, there appears to be a difference between physical processes in the forward and backward directions in time. That is, many of the processes we observe on an everyday basis appear to occur only in the forward time direction, such as in the classic example of an ice cube melting in a hot cup of coffee. However, the fundamental laws of physics are for the most part symmetrical with respect to the direction time: the fundamental equations of physics yield the same predictions in both time directions, and do not change when the variable "t" is replaced with "$-t$". So while from the point of view of everyday human experience there appear to be significant differences between physical processes in the forward and backward directions of time, insofar as certain processes appear to only occur in one direction, such differences vanish at the level of physical laws.

The discrepancy between the human experience of the time asymmetry of physics and the time symmetry of the fundamental laws of physics is an issue that has occupied a wide range of past thinkers, particularly those involved in study of heat in the mid-to-late nineteenth century. As Theodore Porter describes in his 1988 book *The Rise of Statistical Thinking: 1820-1900*:

> That the laws of Newtonian mechanics are fully time-symmetric and hence can equally run backwards or forwards could not easily be reconciled with the commonplace observation that heat always flows from warmer to cooler bodies. This discrepancy became for a time one

> of the deepest theoretical problems of the dynamical – or, as it later came to be regarded, statistical – gas theory.[26]

Within the context of the nineteenth century of study of heat, the directionality of physical processes became codified in the second law of thermodynamics. In 1865 physicist Rudolph Clausius described the second law of thermodynamics as "the universal tendency of entropy to increase,"[27] with entropy understood as a measure of "the uselessness of a certain amount of energy."[28] Thus, during the mid-to-late nineteenth century the discrepancy between the time asymmetric appearance of physical processes and the time symmetry of physical laws was primarily framed as a discrepancy between the laws of thermodynamics and the laws of Newtonian physics.

A possible resolution came with the work of Ludwig Boltzmann in the 1870s, proposing that the macroscopic directionality observed in physical processes is a statistical consequence of the microscopic behavior of atoms.[29] Boltzmann reformulated the second law of thermodynamics to state that physical systems tend to move from states of lower statistical frequency to higher statistical frequency, with entropy understood as "a measure of the number of particular microscopic arrangements of atoms that appear

[26] Theodore Porter, *The Rise of Statistical Thinking, 1820-1900* (Princeton: Princeton University Press, 1986), 193.

[27] Huw Price, *Time's Arrow and Archimedes' Point: New Directions for the Physics of Time* (Oxford: Oxford University Press, 1996), 22.

[28] See Sean Carroll, *From Eternity to Here: The Quest for the Ultimate Theory of Time*, (New York: Dutton, 2010), 34.

[29] *Ibid.*, 36-38.

indistinguishable from a macroscopic perspective."[30] This meant that at the microscopic level the time symmetric Newtonian laws obtained; it was only at the macroscopic level that an asymmetry with respect to time appeared. This development was the source of much controversy in the late nineteenth century, and was the source of debates that involved issues including the nature of time, determinism, and free will. Many of the prominent physicists involved in nineteenth century thermodynamics participated, such as Maxwell and Boltzmann, and each brought his own agenda to the issue. The positions of Maxwell and Boltzmann within these debates has been summarized by Porter as follows:

> Whereas Maxwell stressed the imperfection of statistics and hence of most knowledge in physics, emphasizing the possible existence of instabilities and singularities, Boltzmann took the exceptional improbability of the visible universe and sought to fit it into a pattern of equilibrium conditioned by statistical regularity. Maxwell, while never advocating positive indeterminism or acausality, wished to establish the possibility of a nonphysical causality that depended on the action of the will. Boltzmann, never comfortable with the dependence of science on probability, except in terms of stable frequencies, refused to countenance the idea that the most fundamental phenomena of nature could be other than mechanically determined.[31]

Following Boltzmann's statistical definition of entropy and the second law of thermodynamics, the question of why systems tended to move forward in time toward higher entropy states, as opposed to backward, remained open. Boltzmann proposed one possible solution, suggesting that the universe began in a low entropy state, and that the

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[30] *Ibid.*, 37-38.

[31] Porter, *The Rise of Statistical Thinking*, 216-217.

perceived directionality of physical process is the result of this initial boundary condition.[32] This proposal has been widely accepted by physicists, and is currently believed by most to explain the directionality of thermodynamic processes.[33] Thus, the issue appeared to be largely resolved by the twentieth century. The fundamental laws of physics were thought to be universally symmetric with respect to time, just as they were with respect to space. The appearance of time asymmetry was due to the statistical behavior of microscopic processes, as well as contingent boundary conditions.

When Cronin and Fitch conducted their experiment in the early 1960s, the consensus among physicists was that the fundamental laws of physics were completely symmetrical with respect to time. This meant that at the most fundamental level the laws of physics treated the forward and backward time directions identically, and did not display a directionality in the time dimension that did not exist for the space dimensions. However, as I describe in chapter two, Fitch and Cronin's experiment implied that there were basic laws of physics that were *not* symmetrical with respect to time, even when taking the boundary conditions into account. In other words, their work suggested that the forms of certain physical laws were different in the forward and backward directions of time. This presented the possibility that the classic nineteenth century conversation about the direction of time with respect to physical laws could be reopened. Do physical laws differentiate between the forward and backward direction in time? If so, how should the difference be characterized? How does this speak to the physical relationship between

[32] *Ibid.,* 38.

[33] Price, *Time's Arrow*, 22.

time and space? These are questions Fitch and Cronin identified but did not engage with on a substantive level, believing them to be beyond the scope of their roles as experimental particle physicists. Further, many other physicists, philosophers, and popular writers who were interested in questions about the nature of time were at a loss for how to incorporate Fitch and Cronin's result into their thinking, and largely dismissed it.[34] In chapter two I argue that Fitch and Cronin's discovery is relevant to basic questions about the nature of time, but I also explain why this relevance hasn't been explored in any depth. I claim that within the context of postwar particle physics there was no framework through which the relevance of such a discovery for fundamental questions about the nature of time could be explored.

In chapter two I draw upon works in the history and philosophy of science that analyze the theoretical entities of postwar particle physics, in order to gain insight the status of a concept like the directionality of time. I then use this insight to explain why the philosophical implications of Fitch and Cronin's result have not been pursued. Drawing on works such as Andrew Pickering's *Constructing Quarks*, Peter Galison's *How Experiments End* and *Image and Logic*, and David Kaiser's *Drawing Theories Apart*, I look to how scholars have understood the status of conceptual entities that operated as part of the experimental and theoretical practices of postwar particle physics. Focusing on the presuppositions about time held by members of this community, and the

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[34] For example P.C.W. Davies, *The Physics of Time Asymmetry* (Berkeley: University of California Press, 1974); Brian Greene, *The Fabric of the Cosmos* (New York: Vintage Books, 2004).

relationship between these presuppositions and notions about time and its direction, I will shed light on how the concept of time gained meaning within the context of postwar particle physics. This in turn will be used to show why questions about the implications of Fitch and Cronin's result were not explored by Fitch and Cronin themselves, nor by other thinkers interested in the nature of time, even though the result appears to be relevant to a traditional line of philosophical questioning about time.

Chapter two presents postwar American particle physicists as caught between philosophical and pragmatic tendencies within physics. In keeping with a partially philosophical sensibility, this subgroup was deeply concerned with fundamentals; yet on the pragmatic side, it treated a fundamental concept like time as a technical variable confined to the technical interface of theory and experiment. In this context, the framework of particle physics did not afford a mechanism by which its concept of time could be transported into philosophical discourses about its nature. The chapter looks to the contingencies and values that went into particle physicists' understanding of time as a technical variable, and how it gained meaning within the framework of postwar American particle physics. Drawing on historiography surrounding the theoretical entities conceptualized by the postwar particle physics community, I show the conventions that helped form this understanding of time, and why there was no philosophical consideration of the impact of Fitch and Cronin's experiment for deeper understandings of this fundamental concept.

My analysis of why the philosophical implications of Fitch and Cronin's discovery for the concept of time have not been explored will extend beyond the fact that

Fitch, Cronin, and their colleagues never asked or answered questions about the philosophical implications of the discovery. While it is true that the posing of philosophical questions about time was outside of the scope of this subgroup's self-understanding, the technical nature of the experiment, and they way it pulled different presuppositions about time together, left little room for philosophical discussion. With time cast as a technical variable within the framework in which the experiment was conducted, even self-identified philosophers lacked the resources to dig deeply into the experiment's implications for the direction of time. The concept of time itself, as conceived within the particle physics community, could not be set in relation to philosophical lines of questioning, even if it appeared on the surface to address traditionally philosophical issues. The earlier nineteenth century debates about time and its direction had little meaning for the time as conceptualized in the context of Fitch and Cronin's experiment.

Chapter two builds on chapter one to add a further element to the larger landscape of ways American physicists understood and engaged with the concept of time during the postwar period. It describes physicists who had different understandings of what it meant to be a physicist from those considered in chapter one, with more complicated relationships to philosophical approaches to physics. Further, it shows an additional register on which the concept of time was at issue – that is, its "directionality" – and maps out the contours of this issue. Finally, it shows why, within these contours, there was hardly any philosophical investigation into the nature of time on a fundamental level.

This will shed light on particle physicists' understanding of time as a technical variable, as well as the assumptions that went into this understanding of time.

2.3 Chapter three: Quantum Gravity and the Illusion of Time

Chapter three moves from the large community of postwar experimental particle physics to the significantly smaller subgroup of physicists who conducted research into general relativity in the 1950s and 1960s. Specifically, it focuses on the efforts of several members of this subgroup to bring general relativity and quantum mechanics together to form a universally valid theory of quantum gravity. Questions about the nature of time were explicitly at stake in this subgroup's research program. Specifically, these physicists asked questions about whether time can be said to "exist" at all, frequently making the claim that time does not exist. What did these physicists mean by the claim that time does not exist? Further, what does the fact that they made such a claim reveal about their understanding of the role of the physicist, with respect to the tension between philosophy and pragmatism? Finally, what presuppositions did they make about time, and on what level did they question these presuppositions?

Chapter three explains the willingness of these physicists to engage with the concept of time in two parts. First, it argues that questioning the notion of time was central to the intellectual inheritance of relativity researchers. Questions about the nature of time have played a central role in the study of relativity beginning with Einstein's first paper introducing the special theory of relativity in 1905.[35] In this paper Einstein defined time operationally, as no more or less than the measurements recorded by a system of synchronized clocks. This definition was directly opposed to the previously held Newtonian notion of absolute time. Einstein's definition, together with the two principles of special relativity, yielded the famous and counterintuitive insights into the relativity of simultaneity and time dilation that are foundational to special relativity.[36] These insights placed questions about the nature of time at the center of the original formulation of special relativity. Further, Minkowski's 1908 interpretation of special relativity in terms of a four dimensional spacetime, and Einstein's 1916 geometrical description of gravity in terms of curved spacetime in the general theory of relativity, continued the tradition whereby insights into the nature of time were central to advances in relativity.[37] For

[35] Albert Einstein, "On the Electrodynamics of Moving Bodies," in in *The Principle of Relativity*, ed. H. Lorentz et. al, (Mineola, NY: Dover Publications Inc., 1952), 35-65.

[36] Scholars such as Peter Galison, John Norton, and John Statchel have written extensively about the redefinition of time in the special theory of relativity, noting the central place the redefinition of time played in Einstein's formulation of the from multiple angles. For example, see Galison, *Einstein's Clocks and Poincare's Maps*; John Norton, "Einstein's Special Theory of Relativity and the Problems in the Electrodynamics of Moving Bodies that Led him to it," in *The Cambridge Companion to Einstein,* ed. M. Janssen and C. Lehner (Cambridge: Cambridge University Press, 2004); Michel Janssen, "Drawing the Line between Kinematics and Dynamics in Special Relativity," in *Studies in the History and Philosophy of Modern Physics* 40 (2009): 352-362.

[37] See Hermann Minkowski, "Space and Time," in *The Principle of Relativity*, ed. H. Lorentz et. al, (Mineola, NY: Dover Publications Inc., 1952), p. 75; Albert Einstein, "The Foundation of the General Theory of Relativity," in *The Principle of Relativity*, ed. H. Lorentz et. al, (Mineola, NY: Dover Publications Inc., 1952), p. 109.

researchers working on relativity in the decades leading up to the postwar period, posing questions about the concept of time was central to their intellectual inheritance.

This subgroup was further predisposed to overtly ask questions about the nature of time due to unique challenges that emerged from the task of uniting general relativity and quantum mechanics into a theory of quantum gravity. That is, the concept of time underwent significant changes over the course of the development and interpretation of the theory of relativity, and was understood by relativity researchers as bound up with the notion of the curved four-dimensional spacetime central to Einstein's theory of gravity. As such, it was not immediately obvious that the relativistic concept of time was compatible with the concept of time built into quantum mechanics. This has often been described as the "problem of time" in quantum gravity, and is an issue with which physicists working on quantum gravity have directly grappled. In his paper "Quantum Gravity: A Primer for Philosophers," Dean Rickles has described the problem of time as follows:

> Time is a fixed 'external' parameter in standard quantum theory, a structure against which dynamics unfolds but that is not itself determined dynamically. In quantum mechanics time appears in the fundamental dynamical equation (the Schrodinger equation) as Newtonian absolute time. […] Not so in general relativity where the spacetime geometry will be determined by the state of matter.[38]

Thus, in addition to the fact that questioning the nature of time was part of the intellectual inheritance of the field of relativity, the project of uniting general relativity and quantum

[38] Rickles, "Quantum Gravity: A Primer for Philosophers," 18.

mechanics into a single theory was particularly primed to directly grapple with questions about the nature of time.

In addition to intellectual reasons, the community working on general relativity had a different relationship to the mainstream pragmatic culture of physics than the other subgroups considered in this dissertation. In the 1950s and 60s, the subgroup was quite small, and often found itself on the margins of the mainstream physics community, while still having several prominent members. Thus, the subgroup was better situated to deviate from the pragmatic mainstream of physics than other, larger subgroups of physicists working during this period. Several historians of science have written about the size and status of the community of general relativity researchers in the United States during the twentieth century, including David Kaiser, Jean Eisenstaedt, and Daniel Kennefick. These scholars explain why the subgroup American physicists working on general relativity in the early years of relativity research was small and often marginalized. Further, they show how and why the field of relativity research grew and transformed during the postwar period. Chapter three of this dissertation situates physicists working on quantum gravity within this transitional period in the history of general relativity research, in order to explain how they came to their questions and answers about the nature of time. Although they shared much of the pragmatic sensibility that characterized the practices of many of their colleagues working in other fields of physics, they also felt a greater license to speculate about philosophical questions, as they were closer to the margins of the discipline. In a unique position with respect to mainstream physics, along with the specific historical circumstances in which the community emerged, this subgroup was able to think deeply about the nature of time and make philosophical claims about its very existence.

Chapter three demonstrates that the physicists working on quantum gravity during the postwar period were closer to the philosophical end of the spectrum than many other subgroups during this period; further, it seeks to explain why their more philosophically-minded sensibility was possible. This being said, although the physicists considered in chapter three were more open to asking philosophical questions about their basic concepts than other physicists, these questions were still constrained by their understandings of what it meant to do physics. Even though they understood the boundaries of physics to be more inclusive than did other physicists, they still had a defined sense of these boundaries. Further, the physicists shared a great deal with the mainstream pragmatic culture of physics, and their approaches involved many pragmatic elements. Chapter three thus aims to describe the nuanced understanding of the boundaries of physics that enabled physicists working on quantum gravity to ask and answer questions about time in the specific form in which they did. While doing so the chapter builds on the first two chapters by adding analysis of a further layer of questions and assumptions about the nature of time. By looking to the presuppositions that postwar physicists working on quantum gravity held about time, and how these presuppositions affected the questions they asked and the answers they arrived at about time's existence, a further dimension will be added to the heterogeneous picture of the concept of time in the postwar United States.

3.

This dissertation describes communities of postwar American physicists that treated time differently on multiple registers. First, each subgroup cast time in a different role, i.e. as a unit of measure, experimental variable, or background for change. Further, the work of each raised questions about different characteristics of time, such as its universality, directionality, or ontology. Finally, each used different methods to derive insight into time, including instrumental advances, experimental techniques, and speculative musings. More generally, the subgroups differed on the degree to which they understood interpretive questions about time to be at stake in their work. The dissertation aims to consider all of the above registers of difference, unpacking and explaining the presuppositions about time and physics in which they were embedded. In doing so it will set subgroups of postwar American physicists in relation to one another in terms of their engagement with time, illuminating the boundaries that existed within the physics community during this period. Time will emerge as a heterogeneous and contingent concept situated at these boundaries, simultaneously blurring and rarefying the distinctions between subgroups of physicists.

The dissertation involves two complementary lines of questioning: the first inquires into the professional identities of subgroups of postwar American physicists, and the second asks about the nature and status of time as a fundamental concept within these subgroups. The first uses physicists' various levels of engagement with philosophical questions about time to show how different subgroups of physicists understood their professional roles and the boundaries of their discipline, in terms of the spectrum running from philosophical to pragmatic sensibilities. It shows that different subgroups of physicists engaged with questions about the nature of time to varying degrees, in turn revealing a wide range of conceptions of, and approaches to, physics. In the first chapter, the physicists were firmly rooted within the mainstream pragmatic culture of physics, and were not attuned to fundamental conceptual changes with respect to timekeeping brought about by their work. In chapter two, the physicists were pulled between philosophy and pragmatism, causing them to identify but not pursue basic questions about time. In the third chapter, the physicists were closer to the philosophical side of the spectrum than those considered in the first two chapters, and openly speculated about the nature of time. Nevertheless, even the physicists in chapter three were closely aligned with the mainstream pragmatic culture in many ways, which constrained their philosophical conversations. As part of this first line of questioning, the dissertation will explain the differences between physicists' approaches to the concept of time. This will involve looking to the intellectual, institutional, and cultural inheritances of each subgroup, as well as the historical and technical details of the specific research programs under consideration.

The second line of questioning concerns the concept of time itself, and the ways in which it changed and was conceptualized in postwar American physics. It charts the ways in which the concept of time emerged as a philosophical issue within each subgroup, and the specific ways the concept changed. In chapter one, this involves describing the changes in basic time keeping concepts within the community of physicists working on the development of atomic clocks and atomic time standards. In chapter two, it involves a demonstration of how the results of Fitch and Cronin's experiment presented a potential complication to longstanding debates about the direction of time. In chapter three, it involves describing the line of reasoning that led physicists to the conclusion that time does not exist. This line of investigation seeks to use these moments when the concept of time was at stake to understand physicists' assumptions and presuppositions about time. Beginning with time as a unit of measurement in chapter one, moving through time as a technical variable in chapter two, and ending with time as an object of problematic ontology in chapter three, the dissertation unpacks multiple layers of assumptions and presuppositions about time. Building on this, it aims to contextualize these assumptions and presupposition within the values and contingent circumstances of each subgroup. Finally, in the conclusion this line of questioning will abstract away from the concept of time and look to the nature of fundamental concepts in postwar physics more generally. Considering the variety of presuppositions about time held by postwar physicists, it argues for the contingency, heterogeneity, and necessity of physicists' presuppositions about fundamental concepts, and the role that these presuppositions play in giving physical concepts meaning.

Overall, the dissertation reveals a multiplicity of ways of engaging the concept of time, as well many different presuppositions about time. It paints a picture of time as a historically contingent and heterogeneous concept. Time meant different things to different subgroups of physicists during the postwar period; further, each subgroup made different assumptions about time. This raises the question of whether there were any presuppositions about time that were shared among the subgroups considered. Is it possible that all of the physicists upheld a basic presupposition about a "truth" of time? Even though the physicists in chapter three did not assume time "exists", did they presuppose anything about time before they speculated about its existence?

In the conclusion to the dissertation, I will explore how presuppositions can function as preconditions for physical concepts in a variety of ways. This will be used to form a more general argument about the nature of the concept of time – and conceptualization in general – in physics. In order to do so, I will draw upon the work of several thinkers from twentieth century continental philosophical tradition to help develop insight into the nature of time as a fundamental concept in physics.[39] With somewhat different emphases, these thinkers draw attention to the relationship between the presuppositions that structurally precede concepts – the conditions of possibility for a concept – and the content of a given concept. They discuss the difference between presupposition and content – what they see as the inescapable heterogeneity of a concept in relation to itself – and the consequences of this difference. By drawing on the structures discussed by these thinkers, I will give contour to my own discussion of the meaning of the particular heterogeneity of the concept of time in the history of physics, and the presuppositions that underlie it. By considering the specific, historical contingency of the presuppositions that underlie physicists' concepts of time – together

[39] E.g., Martin Heidegger, *Being and Time,* Trans. John Macquarrie and Edward Robinson (New York: Harper Perennial Modern Thought, 2008), 29-31; Martin Heidegger, "Modern Science, Mathematics, and Metaphysics," in *Basic Writings* (New York: Harper and Row, 1977), 452; Jacques Derrida, *Aporias* (Stanford: Stanford University Press, 1993), 32; Giorgio Agamben, *Language and Death: The Place of Negativity* (Minneapolis: University of Minnesota Press, 1991).

with the more radical heterogeneity of concepts in general as described by twentieth century continental thinkers – I will draw conclusions about the nature of a fundamental concept, like time, in postwar American physics.

The overall aim of this dissertation will be to offer a nuanced account of the landscape of professional identities of postwar American physicists, as well as the way these physicists conceptualized time. Together with the concluding philosophical analysis of conceptualization in general in physics, the dissertation will provide a deeper understanding of physicists' relationships to their most fundamental concepts.

CHAPTER ONE

Atomic Times: Clocks and Time Standards in the Post-War Era

1. Introduction

On January 7th, 1949, the *New York Times* published an article with the headline "Government Makes Atomic Clock Telling Time Better Than the Stars." The article described a clock, unveiled in late 1948 by physicist Harold Lyons and his team at the US National Bureau of Standards (NBS), which used the properties of the ammonia molecule to keep time.[40] The announcement of Lyons' ammonia clock, and the soon to be iconic photograph of the apparatus connected to an ordinary clock face,[41] garnered a great deal of attention both from within the scientific community and from the mainstream American press. One person acutely aware of Lyons' work on the ammonia clock was astronomer William Markowitz, working within the time services division of the US Naval Observatory (USNO). Markowitz had been paying close attention to the progress of atomic clock building, and was worried that the development of atomic clocks would shift control of timekeeping away from "the stars" and toward physics laboratories.[42]

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[40] The ammonia clock is often referred to as an atomic clock, even though it was technically based on a molecule as opposed to an atom.

[41] See figure 1.1.

[42] William Markowitz, Interviewed by Steven Dick, 18 August 1987.

Figure 1.1. Publicity Photo of Lyons with his Ammonia Clock

Although the ammonia clock never did tell time "better than the stars,"[43] and the project was quickly abandoned, it catalyzed efforts by Lyons and others to begin developing a more promising clock based on cesium atoms.[44] Markowitz, believing the development of a cesium clock to be inevitable, felt strongly that astronomers should help physicists develop a cesium time standard compatible with existing astronomical time standards. He approached Lyons in the early 1950s, hoping to collaborate on the establishment of a new atomic definition of the second. Lyons had no interest in collaborating with an astronomer, so Markowitz instead struck up a partnership with British physicist Louis Essen at the UK National Physics Laboratory (NPL). Essen had a cesium clock up and running in 1955 and, through his collaboration with Markowitz, had established the first atomic definition of the second by 1958.

During this ten-year period, from the unveiling of Lyons' ammonia clock in 1948 to Markowitz and Essen's establishment of the first atomic time standard in 1958, remarkable changes occurred in the ways scientists measured, used, and conceptualized time. In the process, scientific communities implicitly renegotiated their understandings of basic timekeeping concepts such as "clock", "second", and "time", proliferating a multiplicity of meanings and usages of these terms. These concepts did not have single, fixed meanings before or after the development of atomic clocks and time standards;

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[43] The ammonia clock was highly unstable, and never more precise than the non-atomic clocks and time standards already in existence. As described in As described in Paul Forman, "Atomichron ®: The Atomic Clock from Concept to Commercial Product," *Proceedings of the IEEE,* **73** (1985), 1184.

[44] *Ibid.*

however, their heterogeneous meanings shifted as a result of practical work carried out in the field of atomic timekeeping from 1948-1958. Such conceptual changes did not occur at the level of explicit discourse – neither physicists nor astronomers overtly discussed the meanings of their basic concepts or the ways in which they were changing. Nevertheless, in this chapter I will show how the advent of atomic clocks and time-standards was accompanied by implicit conceptual renegotiation.

Why did the physicists involved in the development of atomic clocks and atomic time standards fail to explicitly discuss the meanings of basic time keeping concepts? In what follows, I will argue that these physicists were firmly rooted in a postwar, pragmatic professional culture that placed philosophical investigation into the meaning of basic timekeeping concepts outside the domain of physics. I will describe the institutional context in which the physicists working on atomic time were situated - focusing specifically on NBS, NPL, and USNO, which respectively supported Lyons, Essen, and Markowitz - to help understand this professional culture. By considering the projects, mandates, and sources of funding for these institutions, I will outline the ways in which the wartime physics legacy and the postwar context contributed to these physicists' professional self-identities. Further, I will look to the attitudes and personalities of prominent physicists whose work helped pave the way for atomic time, to show how they helped stabilize this pragmatic culture. I will use the professional culture within the institutions supporting research into atomic time, along with the personalities of its model physicists, to explain the absence of philosophical discourse about the meaning of timekeeping concepts among the physicists involved in the establishment of atomic time; conversely, I will use this absence to gain insight into the contours of this culture.

Nearly all of the institutions that supported atomic time research during the post-World War II period were either run or funded by government agencies. Government involvement in this research, within the post-World War II context, was thus one of the major factors that influenced the culture of these institutions, setting the tone and mandate of this research in a variety of ways. The role of the government in physics, and the ways in which physics institutions changed as a result increased government involvement during the Cold War, has been written about extensively by historians of science including David Kaiser, Stuart Leslie, Peter Westwick, Peter Galison, George Reisch, Paul Forman, and many more.[45] In this chapter I will build on these works, showing how the changing relationship between science and government affected work

[45] David Kaiser, "Cold War Requisitions, Scientific Manpower, and the Production of American Physicists after WWII," in *Historical Studies in the Physical and Biological Sciences* **33** (2002), 131-159; Stewart Leslie, *The Cold War and American Science: The Military-Industrial-Academic Complex at MIT and Stanford* (New York: Columbia University Press, 1993); Peter Westwick, *The National Labs: Science in An American System, 1947-1974* (Cambridge, MA: Harvard University Press, 2003); George A. Reicshe, *How the Cold War Transformed the Philosophy of Science: To the Icy Slopes of Logic* (Cambridge: Cambridge University Press, 2005); Peter Galison, *Image and Logic,: A Material Culture of Microphysics* (Chicago:University of Chicago Press, 1997); and Paul Forman, Behind Quantum Electronics: National Security as Basis for Physical Research in the United States, 1940-1960," *Historical Studies in the Physical and Biological Sciences* **18** (1987): 149-229.

on atomic time in the 1950s, contributing to a professional culture of physics that had little room for speculative conversations about the meaning of timekeeping concepts.

In what follows, my primary objective will be to trace the destabilization and renegotiation of fundamental timekeeping concepts during the early years of atomic timekeeping. Further, I will show how this destabilization and renegotiation occurred at the implicit level, due to the pragmatic culture in which the physicists and astronomers working on atomic time were situated. While doing so, I will draw attention to two closely related secondary tensions at play. Both tensions reveal a set of factors that contributed to the establishment of atomic time, showing the many values, contingencies, and agendas that produced the clocks and standards that are now the basis for the most precise measurements in physics. The first is a disciplinary tension between physics and astronomy. This tension manifested itself during debates over how time-keeping labor was to be distributed among physicists and astronomers following the development of atomic clocks. How did physicists and astronomers define their disciplinary roles in relation to one another? To what extent did their visions of these roles overlap, and how did each group stake claims on contested domains? These questions were at stake in early developments in atomic timekeeping, and the tension between physics and astronomy influenced the way atomic clocks and standards were developed in the 1950s.

The second tension existed between those striving toward the ideal of universality in timekeeping and those emphasizing the contingency of time. There was a sentiment among some physicists, such as Harold Lyons, that timekeeping concepts could and should be defined such that they would be true and valid anywhere in the universe. Such a definition would not rely on specific periodic motions as viewed from our particular planet; rather, it would be valid throughout the universe independent of context. Conversely, many astronomers, such as William Markowitz, believed that timekeeping concepts have and always should be tied to our contingent experiences of time on earth, with its particular orbit and axis of rotation.[46] For Markowitz, a definition of the second that was "universal", but unrelated to the human experience of change, would detrimental to the field of timekeeping. Thus, the universality and contingency of time measures were at stake during the early days of atomic timekeeping, revealing another set of factors that influenced the development of atomic clocks and standards. The tension between these values affected the way the atomic second was defined, as did the tension between physics and astronomy, showing again that standards for atomic time measurements are not objective "facts" but rather the product of specific values and agendas.

These two tensions - physics versus astronomy, and universality versus contingency - map closely onto one another: one side involves physicists, striving for a universal atomic time; the other involves astronomers, believing contingent human experience to be of vital importance in defining atomic time. In this chapter I will show that these tensions, along with the meanings of time keeping concepts themselves, were being negotiated among the scientists working on atomic time during in the late 1940s and 1950s. The relationships between astronomy and physics, as well as universality and contingency, were at stake during this period. Further, these tensions reveal the human

[46] Interview of William Markowitz by Steven Dick on 18/08/1987, from James Melville Gillis Library, US Naval Observatory, Washington DC.

agendas, contingencies, and values that contributed to the destabilization of timekeeping concepts during the early years of atomic timekeeping, influencing the way clocks, seconds, and time were conceptualized by physicists.

The humans factors that have influenced the development of standards have been discussed in many interesting works in the history of science, for example by Ken Alder with respect to the development of the metric system in the 18th century, Hasok Chang with respect to the standardization of temperature in the 19th century, and Peter Galison with respect to the standardization of length, time, and weight measures at the turn of the 20th century.[47] These historical accounts contrast the scientific impulse toward objectivity with the human contingencies involved in the establishment of standards. These scholars make the case for why standards should not be taken as objective facts, but rather be understood as the product of historical contingencies; in doing so, they show how the ideal of objectivity is but one of the contingencies that has influenced the production of standards. The standardization of atomic time adds a new element to the ideal of an objective standard unit measure, insofar as atomic standards were grounded in the characteristic frequencies of atoms, and offered a new level of stability and precision to time measurement. Of course, despite this, atomic time was still born of historical contingency. The story of atomic time involves human judgment, decisions, and agendas. Through a discussion of the two tensions described above, along with the technical

[47] Ken Alder, *The Measure of All Things* (New York: Free Press, 2002); Hasok Chang, *Inventing Temperature* (Oxford: Oxford University Press, 2004); Peter Galison, *Einstein's Clocks and Poincare's Maps* (New York: W. W. Norton and Company, Inc., 2003).

details of the specific tools and techniques involved in atomic timekeeping, I will draw attention to some of the contingencies that factored into the development of atomic time standards.

In section two of this chapter, I will briefly discuss a few points regarding the history of clocks and time standards before any serious efforts to build an atomic clock were underway. In section three, I will unpack the story of Lyons, Markowitz, Essen, and their work on atomic clocks and atomic time standards. This will involve discussion of the specific institutions within which they were working. In section four, I will draw conclusions about the ways in which the basic concept of a 'clock', a 'second' and 'time' were at stake in these episodes. This will involve delving deeper into the novel elements of atomic timekeeping tools and techniques, the technical and conceptual challenges they posed, and why and how atomic clocks and standards challenged previously held timekeeping concepts. Throughout, I will draw attention to the ways in which changes in new time-keeping instruments, and new types of standards and techniques, affected timekeeping concepts during the postwar period.

2. Before Atomic Clocks and Standards

2.1 Pre-Atomic Clocks and Time Standards

Before the development of atomic clocks, the distinction between clocks and time standards was relatively straightforward. Time standards established the length of time intervals, and belonged primarily to the domain of astronomy. Astronomers used periodic motions observed in the sky to establish standard lengths of intervals such as the day, hour, and second. Clocks, on the other hand, were physical objects engineered by humans to mark the passage of time on earth, and were more closely connected to the domains of physics and engineering. Clocks marked or measured time, their rate depending on the properties of a specific clock, for example the length of a pendulum string or the cut of a quartz crystal. Physical clocks were not 'universal,' which is to say no two pendulums or quartz crystals were exactly alike; this meant that basing a time standard on a specific clock would be unwieldy, and there were in effect no physical time standards. Physicists and engineers calibrated their clocks in keeping with the time standards established and disseminated by astronomers.

The tension between astronomy and physics surrounding timekeeping has a long history. For example, it was pronounced in the 19th century search for longitude, which raised the question of whether astronomical measurements or precise physical clocks would be more successful in establishing longitude accurately.[48] Further, quartz-crystal clocks, developed in 1927, confirmed scientists' suspicions that the rotation of the Earth was unstable. This spurred an effort for an increasingly stable astronomical standard that could compete with the best clocks in terms of accuracy.[49] Measurements made with quartz clocks went a long way toward destabilizing established definitions of the second and the nature of time measures in general. By casting doubt on the sufficiency of the rotation of the Earth to establish a reliable time standard, the quartz clock played an

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48 For description of the events surrounding longitude and Harrison see, for example, Dava Sobel, *Longitude: the True Story of a Lone Genius Who Solved the Greatest Scientific Problem of His Time* (New York: Walker Publishing Company, 2005).

49 *Ibid*., 107.

important role in sending the concept of the second, as a standard measure of time, into flux. Quartz clocks didn't offer an alternative time standard, however; they rather demonstrated that the standard based on the rotation of the Earth was problematic. This ultimately led to the development of the ephemeris time standard, discussed in detail in section 3.3 below, which was defined in terms of the motion of the earth around the sun, as opposed to the earth on its axis. The development of quartz clocks, and the discovery that the rotation of the earth was not stable, set the stage for conceptual changes precipitated by the advent of atomic clocks and time standards.

2.2 The Beginnings of Atomic Time

The idea that atoms could keep time and serve as a time standard can be traced to the 19th century writings of James Clerk Maxwell, famous for his work on electromagnetism. In 1873 Maxwell published a paper in which he noted that the frequency of the vibrations of atoms and molecules are 'universal' and could, in theory, be used as a time standard.[50] Due to the universality of the properties of atoms and molecules, Maxwell noted that it was possible for these properties to form the basis of a definition of a second.[51] Sir William Thompson made an observation along similar lines in 1879, claiming that "the time of vibration of a sodium particle corresponding to any one of its modes of vibration is known to be absolutely independent of its position in the universe, and will probably

[50] James Clerk Maxwell, "Molecules," *Nature* (Sept., 1873), 437-411

[51] *Ibid.*

remain the same so long as the particle itself exists."[52] It was not until the twentieth century, with the development of quantum mechanical technique of molecular beam resonance advanced by physicist I.I. Rabi at Columbia University, that physicists began to see atomic clocks, and eventually an atomic time standard, as a practical possibility. The molecular beam resonance method involved inducing transitions between two energy states of an atom or molecule by exposing a beam of particles to applied radiation at a given frequency. When the applied frequency matched the frequency of photons emitted or absorbed in the transitions, a resonance effect was produced. The applied radiation could then be fine-tuned to produce the maximum resonance effect, and in this way function as a standard for the transition frequency.[53]

In 1945 the *New York Times* reported I.I. Rabi's suggestion that his molecular beam resonance method might one day be used to develop a highly accurate clock based on the properties of the cesium atom.[54] From the early days of Rabi's research into molecular beams, he and his colleagues were aware that the molecular beam resonance method could be used to establish a frequency standard as well as a standard for the strength of magnetic fields.[55] Rabi himself wasn't interested in building clocks;

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[52] Sir William Thompson and Peter Guthrie Tait, *Treatise on Natural Philosophy* (Cambridge: Cambridge University Press, 1867), 227.

[53] For a detailed description of the molecular beam resonance method, See John S. Rigden, *Rabi: Scientist and Citizen* (New York: Basic Books, 1987).

[54] William Laurence, "Cosmic Pendulum for Clock Planned," *New York Times*, Jan. 21, 1945.

[55] See Norman Ramsey, "History of Atomic and Molecular Standards of Frequency and Time," *IEEE Transactions on Instrumentation and Measurement* **21** (May, 1972): 90-99.

nevertheless, his work on molecular beam resonance laid out the foundation for all future work on atomic clocks.[56] This being said, it was not obvious to physicists at the time what was needed to happen in to transform a molecular beam resonance device into a clock. Frequency is a measure of cycles per second, and so a concept of time was already built into the basic notion of a frequency standard. How could this concept be used to create a clock, or else a time standard? Was a frequency standard itself a type of atomic clock or time standard? Using the techniques involved in molecular beam resonance to build a clock was entirely new territory in the late 1940s and 1950s, and there was no consensus opinion surrounding what differentiated an atomic clock from a frequency standard. As physicists began to think about the possibility of using the molecular beam resonance method to build an atomic clock, the notion of a clock was implicitly called into question.

Prior to the development of atomic time standards, physicists had made efforts to connect length standards to the characteristic properties of matter. Albert Michelson determined the length of the meter in terms of wavelengths of electromagnetic radiation in the late nineteenth century, using the interferometer he had built to search for the "ether drift" in the famous Michelson-Morely experiment.[57] In the 1950s, when the first atomic clocks were being built, the Bureau International de Poids et Mesures was in the process of officially redefining the meter in terms of the wavelength of electromagnetic

[56] See John S Rigden, *Rabi: Scientist and Citizen* (New York: Basic Books, 1987).

[57] Michelson's work on the meter is described in Robert Crease, *World in the Balance: The Historic Quest for an Absolute System of Measurement* (New York: W.W. Norton & Company, 2011), 203-209.

radiation emitted from Krypton-86, based on Michelson's technique.[58] However, establishing a definition of the second in terms of the characteristic properties of atoms would prove much more complicated than the analogous task for the meter. The molecular beam resonance method appeared best suited to the task, yet fulfilling this task involved many complex elements. In the first attempts to build an atomic clock, it was not clear how a time standard would be circumscribed within the molecular beam set-up. Molecular beam resonance was a new technique, founded on the principles of quantum mechanics, and far from perfect in the 1950s. Developing an atomic clock and atomic time standard based on molecular beam resonance techniques presented a new set of technical and conceptual issues that challenged traditional understandings of timekeeping concepts.

In addition to Lyons' group at NBS, and Essen's at NPL, there were many groups who worked on the development of atomic clocks during the years following World War II, all indebted to Rabi's molecular beam resonance work, and almost all having personally worked with Rabi at some point in their careers. These included groups led Jerrold Zacharias at MIT, Charles Townes at Columbia University, and Norman Ramsey at Harvard University. Rabi was a prominent figure in 20th century American physics, and played a role in creating and disseminating what I have been referring to as the pragmatic culture of postwar American physics. John S. Rigden begins his celebratory biography of Rabi with the line, "When Isidor Isaac Rabi retired from Columbia

[58] *Ibid.*

University in 1968, he embodied the spirit of American physics."[59] Rigden describes how although Rabi was an experimentalist, he also carried out theoretical work, and to a great extent exemplified the ideal of the union of theory and experiment in American physics. Rigden presents a picture of Rabi as deeply interested in practical applications of his work, and committed to contributing to society through science. Further, Rigden makes the case that Rabi's pragmatic sense of professional purpose had a far-reaching impact on the culture of postwar American physics. Rigden describes how on the occasion of Rabi's retirement from Columbia in 1967, many prominent physicists spoke of the extent of Rabi's impact on the field. For example, Rabi's former colleague Jerrold Zacharias presented a "Rabi Tree," which displayed the deep, pervasive lines of Rabi's influence.[60]

Almost all of the physicists involved in early work on atomic clocks and standards worked directly with Rabi. They shared a common orientation to physics, involving a vision of what it meant to be a physicist embodied by Rabi, who served as a mentor and model for many members of the profession. It was within the framework of this vision that Lyons, Essen, as well as Markowitz began their efforts to develop an atomic clock and atomic time standard.

[59] Rigden, *Rabi: Scientist and Citizen*, 3.

[60] *Ibid.,* 9-10. As Rigden describes, the Rabi tree was original conceived in 1958, in a project commissioned by the Office of Naval Research to make the case for the importance of basic research. The case was partially made through the example of the impact of Rabi's work, and the tree was made to illustrate this case.

3. Lyons, Essen, and Markowitz

3.1 Lyons and the Ammonia Clock

One of the first people to directly apply themselves to the task of building an atomic clock was Harold Lyons, working at the US National Bureau of Standards (NBS) during and after World War II. Harold Lyons was trained as a physicist, earning his PhD in nuclear physics from the University of Michigan in 1939. After receiving his degree, he worked for the Naval Research Laboratories for two years before joining NBS in 1941. Upon joining NBS, Lyons worked in the frequency standards division created for radar purposes during the war. As radar frequencies were pushed into the microwave region, a specific microwave frequency standards division was created in 1944. Harold Lyons was made head of this microwave standards division, which continued to operate after the war.[61]

The National Bureau of Standards was founded in 1901, as the US federal government's first physical research laboratory.[62] The institution was created to establish and provide standards to US science and industry, and function as an adjunct of the federal government.[63] During the first fifty years of its operation, scientists at NBS carried out a wide range of projects, involving standards for electricity and railways,

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[61] Biography of Harold Lyons, included in Harold Lyons Atomic Clocks Collection, Archives Center, National Museum of American History, Smithsonian Institution.

[62] Rexmond Canning Cochrane, *Measures for Progress: A History of the National Bureau of Standards* (U.S.: National Bureau of Standards, U.S. Department of Commerce, 1966), 49.

[63] *Ibid.*

public utilities, industrial materials, radio waves, and many other applications. [64] NBS aided the American efforts in both world wars; during World War II, one of its major projects involved the standardization of frequencies used in radar, as well as the effects of various weather factors on radar signals.[65] Radar work carried out in Lyons' microwave standards division of NBS during the war often involved the ammonia molecule, as the transition frequency between two polarizations of the molecule fell in the microwave region of the frequency spectrum. After the war, Lyons applied himself to the development of an ammonia clock based on these microwave transitions, which he unveiled to the public in 1948.[66]

Lyons' ammonia clock involved a thirty-foot copper tube filled with ammonia gas, with fifty percent of the molecules in one polarization and fifty percent in another. Microwaves were sent through one end of the tube and detected by a sensor at the other end of the tube. If the applied frequency corresponded to the transition frequency between the two polarizations, the molecules in the lower energy state would absorb a photon and jump to the higher state. If a photon was absorbed, it would not reach the other end of the tube. The resonant frequency thus corresponded to the applied frequency that resulted in the lowest photon intensity at the end of the tube. When the resonant

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[64] *Ibid.*

[65] See David R. Lide, *A Century of Excellence in Measurements, Standards, and Technology* (US: CRC Press, 2002), 2-3.

[66] Lyons

microwave frequency was reached, it was used to tune a quartz crystal oscillator, which produced the electrical pulses that counted off seconds.[67]

The principle behind Lyons' clock was similar in many ways to that which governed future atomic clocks. It involved the tuning of an applied frequency to the frequency of radiation emitted during a quantum change of state, through the establishment of a resonance effect. At a basic level it was a frequency standard for photons absorbed or emitted during a transition between the two ammonia polarizations. The frequency of photons emitted in the ammonia transitions was known to be 23,870 cycles/second; thus, after resonance was established through the tuning process, the applied frequency was known to a high degree of precision.[68] In theory, if one could then count the number of cycles in the applied radiation in a given time interval, one could determine how many seconds had passed. Thus, another tuning process was necessary to connect the ammonia device to an object, like a quartz clock, that could count seconds in a measurable way. This raises the question: at what point does a device like the ammonia clock become a clock? In a process that involves fine-tuning in two instances – first of the applied frequency to the transition frequency, and then of the applied frequency to a counting device - at what point in the process can time be said to have been measured? As we will see in what follows, a further level of complexity was added when this type of

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[67] For technical details on the ammonia clock, see Norman Ramsey, "History of Atomic and Molecular Standards of Frequency and Time," *IEEE Transactions on Instrumentation and Measurement* **21** (May, 1972): 90-99.

[68] See "Spectral Lines as Frequency Standards," *Annals of New York Academy of Sciences* 52 (1952): 831-871.

device was tuned to an existing time standard. As will be discussed in greater detail in section four, the techniques used in the first atomic devices like the ammonia clock complicated questions about the nature of time measurement, as well as the establishment and maintenance of a standard unit of time, within the context of a system of resonance and fine-tuning.

Lyons put a great deal of effort into the presentation of his ammonia clock, which was unveiled to the public in 1949. The public perception of the clock was important to him, as he highly valued the legacy he created as a physicist.[69] As Lyons had hoped, the clock received a lot of media attention, often involving the iconic publicity photo in which the tube containing ammonia was wrapped around a traditional clock face.[70] The headlines about the ammonia device often presented Lyons' clock as a triumph over nature on the part of scientists, rendering natural time obsolete. For example, on January 7, 1949 the *Chicago Tribun*e ran the headline "An 'Atomic Clock' Promises to Outdo the Earth on Accuracy" and the *New York Times* ran many headlines along the same lines, including the December 28, 1948 headline "Atomic Clock Truer than Earth Rotation".

Lyons' clock is often referred to as the first functional atomic clock. It has also been described as an "embarrassment" and a "failure".[71] This is because the ammonia clock was far less accurate than other already existing clocks, and never ran for more than

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[69] As described in Paul Forman, "Inventing the Maser in Postwar America," *Osiris* **7** (1992): 105-134, 109.

[70] See figure 1.1

[71] See, for example, Paul Forman, "Atomichron ®: The Atomic Clock from Concept to Commercial Product," *Proceedings of the IEEE,* **73** (1985), p. 1185.

a few hours. Discouraged with the ammonia clock project, Lyons and his team at NBS dedicated all of their future efforts toward building a cesium beam device that would function as a clock.[72]

3.2 Essen and the Cesium Clock

Louis Essen joined the UK National Physics Laboratory (NPL) in 1928 and worked extensively on radio frequency, specializing in microwave synthesis during World War II. The British government founded NPL as an attempt to forge a relationship between the government and scientific research. At the opening in 1902, the Prince of Wales made the following comment, expressing the original mandate of NPL:

> *I believe that in the National Physical Laboratory we have the first instance of the State taking part in scientific research. The object of the scheme is, I understand, to bring scientific knowledge to bear practically upon our everyday industrial and commercial life, to break down the barrier between theory and practice, to effect a union between science and commerce.*[73]

Over the first half of the twentieth century, NPL was involved in many projects, including applications of aerodynamics, materials testing, and computing.[74] It was a vehicle for state funded projects, practical applications of theory, and theoretical research with the hope of practical applications.[75] In 1935, NPL was involved in the invention of

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[72] *Ibid.*

[73] See http://www.npl.co.uk/about/history/

[74] *Ibid.*

[75] *Ibid.*

radar, and was home to a frequencies standards division during World War II, in which Essen worked.[76]

During the late 1940s and early 1950s, Essen became extremely interested in Lyons' atomic clock projects, and traveled to the US several times to learn more about the atomic clock programs that were underway at the time. As he wrote in his autobiographical notes:

> I was naturally interested in [atomic clock] developments although spectroscopy was so far out of my field that I did not expect to take an active part, that is, until I visited the USA in 1950 and saw the work at MIT and Columbia University. Zacharias at the MIT was quite enthusiastic and although he was not interested in clock making himself he was confident that his technique could be developed to form the basis of a time standard.[77]

Encouraged by what he learned during his US visits, particularly with the progress being made on cesium devices, Essen decided there was no reason why he couldn't build a cesium clock himself.[78] In the early 1950s he worked with his colleague Jack Parry to build a cesium beam clock, which was successfully running by 1955. Lyons had not yet completed a cesium device.

Essen and Parry's cesium clock involved a beam of cesium atoms exposed to applied radiation at the transition frequency between two energy states of the cesium atom, which induced the transition within the atoms. At the end of the beam, a magnet

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[76] *Ibid.*

[77] Louis Essen, *Time For Reflection*, "The Atomic Clock," 1996. Available at: http://www.btinternet.com/~time.lord/TheAtomicClock.htm .

[78] *Ibid.*

filtered out those atoms that had not undergone transitions and a detector then measured how many atoms had changed states. The stronger the signal at the detector, the closer the applied radiation to the transition frequency. A signal was then sent from the detector to the radiation source, locking it at the frequency that produced the strongest signal. The radiation at the locked frequency could then be used to calibrate another object, like a quartz crystal, which could count seconds.[79]

3.3 Markowitz, Essen, and the First Atomic Second

William Markowitz, an astronomer by training, joined the time services division of the US Naval Observatory (UNSO) in 1936. He become the director of the Time Services Department as USNO in 1953 and remained at this post until his retirement in 1966. During Markowitz's tenure, the mandate of the Time Service department at USNO was to provide precise time to the United States Department of Defense.[80] Although civilian scientists ran many of the other major observatories in the world, a team of naval officers, to whom Markowitz reported, ran USNO. According to Markowitz, there was never any discussion of switching USNO to scientist leadership, and Markowitz found its ties to the Department of Defense entirely unproblematic.[81] Markowitz understood his job to be the keeping of time in the way best suited to benefit the US military and the American people. Markowitz's attitude toward his work was formed in the context of the USNO,

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[79] *Ibid*.

[80] USNO interview of Markowitz by Dick.

[81] *Ibid.*

which was committed to practical applications of timekeeping. His work involved providing standard time to the US military and civilians, and his focus was on the usefulness of the time signals he supplied.

Upon learning of various projects to build an atomic clock in the late 1940s, Markowitz became increasingly worried that physicists might develop a time standard that would not suit the interests of the astronomical community. He felt physicists did not understand the concerns of astronomers when it came to standards and the measurement of time, and feared that physicists would invent an entirely new time standard bearing no relationship to the astronomical standard. Further, he was worried that this would lead to the simultaneous use of multiple time standards, resulting in confusion and error. Finally, he felt that astronomical time was too embedded in everyday experience to be discounted by physicists when developing an atomic time scale.[82] As he later reflected, "We live by the sun, we wake up and go to sleep by the sun, and astronomical time wasn't going to go out of style very rapidly. It was better to have astronomical and atomic time related as closely as possible."[83]

As Lyons and others made progress in their work on cesium clocks, Markowitz became increasingly concerned with how physicists would define the atomic second. In the early 1950s Markowitz approached Lyons about the possibility of collaborating, but Lyons was not interested. As Markowitz later recounted:

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[82] William Markowitz, autobiographical notes, unpublished, held in James Melville Gillis Library, US Naval Observatory, Washington DC.

[83] *Ibid.*

It's very interesting that at that particular time, I couldn't get the interest of the Americans on the necessity for getting the frequency of cesium in terms of the [astronomical] second. […] I approached the Bureau of Standards. Dr. Lyons was in charge of the projects on the first atomic clocks. He just didn't understand and said, ‘Our time is going to be so accurate, we don't need the astronomical time.’[84]

Markowitz approached other physicists as well, hoping to collaborate, but none were interested. As he put it,

> Dr. Townes was not interested, he told me, in trying to obtain the frequency of cesium. He was a physicist and wanted to make a device that was highly accurate and could be used for various purposes in physics, and didn't have the time, facilities, or inclination for making measurements. Of the people I talked to in the U.S. none had the experience with time or frequency and didn't see what the problem was; they just knew that the atomic clock was going to be of superlative excellence. When I went to MIT and talked to Dr. Zacharias, and saw the equipment being made and what was proposed, it seemed to me that they were on the verge of making an atomic clock, but again I could not get anyone to make measurements once the atomic clock was built.[85]

After being rebuffed by the American physicists working on clocks, Markowitz approached Essen at NPL in 1955, who was eager to collaborate.[86] As Markowitz describes:

[84] *Ibid.*

[85] *Ibid.*

[86] *Ibid.*

> Not having had in getting any luck with an American physicist with a clock nearing completion who would cooperate with ephemeris time, I then tried my luck in Europe. Around June 1955 I heard the news that Dr. Essen and Parry had successfully achieved the resonance frequency of cesium. In August 1955 the IAU held its triannual meeting in Dublin Ireland. I met with Dr. Essen and said "I understand you have a cesium oscillator working". He said yes, and I said […] I propose we work together jointly, the Naval Observatory and the National Physical Laboratory, to get the frequency of cesium in terms of the ephemeris second. He agreed and the collaboration of the two institutions started then.[87]

Before Essen finished his cesium device, Markowitz had worked furiously on developing a special technique to establish quickly and accurately what is referred to as “ephemeris time”, which by the 1950s was the most precise astronomical time standard in use. Ephemeris time derives from the motion of the earth in its orbit around the sun, defining the second as 1/31 566 925 of the tropical year.[88] Over the course of the first half of the twentieth century, astronomers had begun to favor ephemeris time over universal time, universal time being based on the revolution of the earth about its axis. This was primarily because precise quartz clocks had shown that the rotation of the earth was slowing down consistently, speeding up from time to time, and by no means stable. Ephemeris time is effectively a measure of the variable “t” in Newtonian mechanics as applied to planetary motions, and therefore observations of the motion of any orbiting object could be measured to extrapolate the time of the earth’s orbit around the sun. In the early 1950s, observations of the motion of the moon around the earth were used to

[87] *Ibid.*

[88] A tropical year is the measure of time from one spring equinox to the next.

determine ephemeris time, as the moon's relatively high speed allowed for the most precise results. Markowitz believed that if the atomic second was to be based on an astronomical time standard, it had to be calibrated to the ephemeris second and not the universal second.[89]

Markowitz felt the urgency of calibrating any future atomic time standard to the ephemeris time standard; however, this first required that the ephemeris time standard be sufficiently established. In the early 1950s the process of determining ephemeris time was a long and drawn-out process. In order to measure the motion of the moon, photographs were taken to determine the moments when particular stars were eclipsed by one edge of the moon. However, the techniques used to photograph the moon had serious technical limitations, requiring a vast number of photographs in order to produce reliable results. It generally took nearly three years from the time of an initial photograph to draw any conclusions. Markowitz could not wait that long - he needed a quick and transmittable ephemeris time standard before the first highly accurate cesium clock was built. In order to solve this problem, Markowitz invented a new type of camera, referred to as the 'moon camera,' that could take highly accurate pictures of the moon in relation to the stars.[90] The result was that far fewer pictures were needed to calculate ephemeris

[89] USNO interview of Markowitz by Dick.

[90] See Figure 1.2 for a picture of Markowitz with his moon camera. The camera allowed for a long exposure time without trailing the moon or the stars as they moved through the sky. The long exposure time allowed more distant stars to show up in the pictures. Further, the camera was able to filter out some of the light from the moon, which often prevented the fainter stars from being showing up in the pictures. As described by William Markowitz in interview Steven Dick, 18 August 1987.

time, eliminating the three-year wait. Markowitz invented the moon camera with the primary purpose of determining the ephemeris time standard with enough precision and consistency to calibrate a cesium clock to ephemeris time.

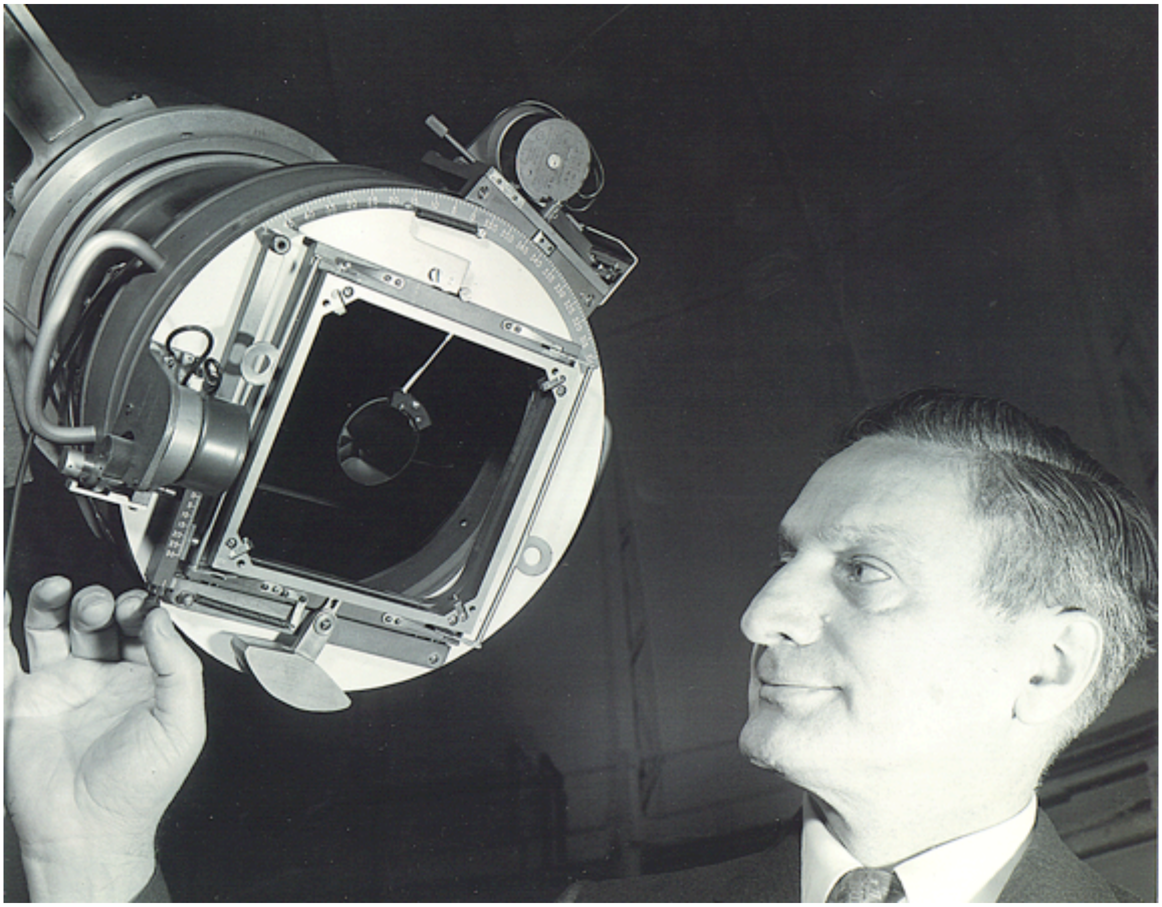

Figure 1.2: William Markowitz with his moon camera

Once Essen's cesium standard was up and running in 1955, Markowitz and Essen applied themselves to the task of calibrating the device to the ephemeris time standard established with the pictures taken with Markowitz's moon camera. To do so, Markowitz communicated ephemeris time to Essen through radio signals, and Essen used the radio signals to determine the fluctuations in the universal second according to ephemeris time. Over the same period, Essen determined the fluctuations in the universal second using the cesium frequency. Both the cesium frequency and the ephemeris frequency registered identical fluctuations in the universal second, which for Essen and Markowitz confirmed the stability of the ephemeris second. It was then a matter of calibrating the cesium frequency to ephemeris time. In 1958, Essen and Markowitz published their results in *Physical Review* Letters, stating that "a second of ephemeris time was found to equal 9,192,631,770 cycles of the cesium frequency."[91] With this number, the second of the first atomic time standard was defined. It is the standard value of the atomic second still in use today.

The collaboration between Markowitz and Essen to establish the first atomic standard was influenced by Markowitz's specific agenda with respect to the future of astronomy and astronomical time standards in timekeeping. Markowitz believed that the atomic time standard needed to be defined in terms of the most current astronomical value, and this belief played a role in giving the standard atomic second the precise form it has today. Markowitz's belief was founded in his disciplinary allegiance to astronomy,

[91] William Markowitz et. al., "Frequency of Cesium in Terms of Ephemeris Time," *Physical Review Letters*, (August 1, 1958), 106.

as well as his belief in the necessary connection between time and human experience. This set of beliefs led Markowitz to develop the techniques needed to for an easily obtainable and stable ephemeris standard, which ensured that the second was defined in accordance of the most current astronomical techniques. For Markowitz, keeping the second tied to an astronomical standard meant the second remained in keeping with an understanding of time in relation to human experiences of change. While not as immediately connected to everyday experience as change between light and dark based on the rotation of the earth on its axis, ephemeris time was still tied to the motion of the earth. Ephemeris time involved a classical notion of time, based on Newton's equations, which presupposed a fundamental relationship between the Newtonian variable "t" and planetary motions. It was tied to the motion of the earth, and thus to human experience of change. Atomic timekeeping presupposed a relationship between "t" and electromagnetic frequencies. By calibrating atomic time to ephemeris time, the classical notion of time, in relation to planetary motion, was kept in harmony with the new atomic notion.

The establishment of the atomic second involved the tuning of several elements to one another. The applied electromagnetic frequency was tuned to the frequency of photon emissions from transitions within cesium atoms, through the establishment of resonance and a feedback loop. This was in turn tuned to a quartz crystal. Further, this whole apparatus was tuned to the radio signals Markowitz used to distribute ephemeris time. This process suggests a series of conceptual questions. Is the concept of "time" consistent in all of the elements being brought together? Further, at what point in the series of calibrations is a measure of time actually determined? These questions will be explored in more detail in section four below, as I draw conclusions about different ways in which timekeeping concepts were at stake during these early days of atomic time.

3.4 Institutional Context for Atomic Time

Lyons, Essen, and Markowitz all conducted their research within institutions supported by government agencies. In each case, the institutional mandate involved providing society with useful scientific applications. These institutions fostered a particular understanding of the purpose of science, which influenced the professional identities of the scientists working within them. In addition to the specific mandates of each institution, which were established well before World War II, the pragmatic cultures of each institution became further entrenched during wartime. Each institution was mobilized in the war effort, particularly with respect to radar research, and the success of this effort contributed to the pragmatic attitudes of the scientists involved. These scientists understood themselves to a certain degree as scientist-citizens, according to the example set by Rabi, and in this capacity were not interested philosophical or speculative issues surrounding their fundamental concepts.

Nearly all of the scientists who worked on early atomic timekeeping personally worked on radar research during World War II. The MIT Radiation Laboratory was turned into the MIT Research Laboratory of Electronics following World War II, which is where Jerrold Zacharias worked to develop the Atomichron, his compact and transportable atomic clock.[92] Atomic clock development at NBS also emerged out of research programs developed for frequency standardization for radar purposes during the war.[93] Further, the National Physics Laboratory, where Louis Essen built his clock, played an important role in the invention of radar.[94] All of the physicists working on atomic time worked on wartime radar projects that were continuous with atomic clock projects. The story of the development of atomic clocks is firmly rooted in postwar physics, with atomic clocks developing as a practical extension of wartime radar research. The scientists involved in atomic timekeeping belonged to a culture built around radar work, which was bound up with the widespread influence of Rabi's approach to physics. As a result, these physicists weren't interested in philosophical puzzles as had been other groups, generations, and communities of physicists.[95] Fundamental theory was important to many of them, but this importance was joined with the possibility of practical applications. These physicists did not see speculative questions like "what is time?" as belonging to their projects. Yet as section four below will discuss, despite the fact that these physicists weren't interested speculating about the nature of

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[92] See As described in Paul Forman, "Atomichron ®: The Atomic Clock from Concept to Commercial Product," *Proceedings of the IEEE,* **73** (1985)

[93] See See David R. Lide, *A Century of Excellence in Measurements, Standards, and Technology*, US: CRC Press, 2002.

[94] See http://www.npl.co.uk/about/history/

[95] See David Kaiser, *How The Hippies Saved Physics*, New York: W. W. Norton & Company, 2011.

fundamental concepts, their work brought about implicit shifts in their discourse about the nature of time.

4. Atomic Timekeeping Concepts

The development of atomic clocks significantly altered the conceptual landscape of timekeeping. Atomic clocks were different in kind from older forms of clock, due to their potential to simultaneously act as time standards and time measuring devices, as well as the principles of resonance and fine tuning on which they were based. Atomic clocks blurred the boundary between frequency standards, time standards, and clocks, resulting in a change in the how these entities were understood. In what follows, I will focus in on the ways in which three timekeeping concepts – "clock", "second", and "time" – were called into question during this period. By doing so, I will show the different assumptions that various physicists and astronomers made about the nature of time and timekeeping in the late 1940s and 1950s. Through this, I will reveal a multiplicity of understandings of the concepts of "clock", "second" and "time" in an atomic context, as well as the presuppositions that conditioned these timekeeping concepts.

4.1 What is a Clock?

During the early years of atomic timekeeping, a form of priority dispute unfolded surrounding the question of who built the first atomic clock. This dispute was fueled in part by the fact that different actors had conflicting ideas about what makes an atomic clock a clock, disagreeing over which device counted as the first atomic clock. Lyons and

Essen's devices were only two of many that were described as the first atomic clock: candidates included the first established frequency standard, the first frequency standard involving a feedback loop, the first frequency standard that could count seconds, the first frequency standard that was self-contained and transportable, and the first frequency standard that operated as a time standard. Atomic clock technology put the concept of a clock in flux, requiring a renegotiation of the meaning of "clock" in an atomic context. Lyons and Essen both believed they had built the first atomic clock, and when weighing in on the priority question often accused one another of not understanding the stakes. For example, Lyons accused Essen of not understanding the difference between a clock and a time standard, claiming Essen had built a time standard and not a clock.[96] To underscore the fact that Essen did not understand the difference between a clock and a standard, he described a meeting he and Essen had both attended as follows:

> Essen proposed at this meeting that a quartz clock could be developed as a primary standard exceeding any other development. Since this is a macroscopic standard whose frequency depends on the particular quartz crystal[…] this proposal betrays a lack of understanding of what a standard should be.[97]

Essen himself spoke as though he had built the first clock, describing his completion of clock as "the birth of atomic time, because much to our surprise it was another year

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[96] Harold Lyons to Paul Forman, 6 May 1985. Harold Lyons Atomic Clocks Collection, Archives Center, National Museum of American History, Smithsonian Institution, Washington, D.C.

[97] *Ibid.*

before any [atomic] clocks were working in the USA."[98] Further, historian of science Paul Forman, who spent many years researching the history of atomic clocks, believed that neither Lyons nor Essen's devices counted as clocks. In a survey paper of the history of atomic clocks, Forman wrote,

> Never was the NPL cesium-beam apparatus – any more than the NBS – operated as an atomic clock. Nonetheless, it is not without some reason that it is commonly cited as the first cesium clock. By establishing a value for the cesium frequency with all the precision of the most advanced astronomical time scales, Essen met the first pre-condition for the establishment of an atomic standard of time interval that could challenge and displace the astronomical.[99]

The question of who invented the first atomic clock illustrates the lack of consensus over the nature of a clock during this period. The idea of an atomic clock was still novel, and whether or not a particular atomic device counted as a clock was up for debate. The blurred boundaries between clocks, frequency standards, and time standards partially resulted from the fact that atomic clocks did not function like other clocks; with multiple levels of tuning, resonance, and feedback, it was unclear which components were necessary for a clock to exist, or where the essence of a clock lay within the system. There was no clear answer to the question of who invented the atomic clock, because at

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[98] Essen, *Time For Reflection*.

[99] Forman, "Atomichron ®: The atomic clock from concept to Commercial Product," 1189. Forman believed Jerrold Zacharias' clock to be the first atomic clock, because it was compact and transportable. Harold Lyons disagreed in 1985 correspondence with Paul Forman, arguing that Zacharias had made an advance engineering but not physics.

the time when the first atomic clocks were being built, there was no definitive concept of what an atomic clock was. This being said, the physicists and astronomers involved did not ask the question “what is a clock”, even though the instruments they used for their most basic and precise measurements were being called into question. This was partially due to their pragmatic professional identities, in the postwar context of the government institutions in which they worked. As a result, they accused one another of being misguided, and often spoke past one another. As opposed to thinking about how the concept of a clock needed to be reconsidered, they accused one another of not properly understanding the nature of timekeeping.

4.2 What is a Second?

Along similar lines, debates over how to define the atomic second complicated the way physicists and engineers understood the nature of the basic unit of time. What factors needed to be taken into account when developing a new definition of the second, and why? What, if anything, is essential and/or universal about the second? Are time measures inherently linked to the cycles of days and seasons as experienced on earth? As this chapter has shown, Markowitz believed it was important to keep the definition of the second connected to the astronomical second, which was tied to the contingent human experience a of time on Earth. Lyons, on the other hand, was less concerned with such contingencies, believing accuracy to be the only major concern when developing a time standard. For a physicist like Lyons, time standards could be decoupled from contingent human experiences and could be established solely through resonance effects. Lyons and

Essen's positions on this point reveal two different visions of how a time standard could and should be defined. The nature of a second, and the meaning and purpose of a time standard, needed to be renegotiated in the context of these two positions.

The development of atomic clocks transformed the concept of a time standard, and in turn the concept of a second. Atomic clocks combined time standards and time measuring devices together in a single physical object, blurring the line between a time standard and a clock. Further, these devices functioned as time standards through the establishment of resonance, feedback loops, and the fine-tuning of an applied frequency to a known frequency. This system was different in kind from the processes used to extrapolate a time standard from astronomical observations. To what extent were the two processes continuous, and to what extent did an atomic time standard represent an entire new concept of a measure of time? This question was at stake in the 1950s and 60s, as physicists and astronomers forged out a new atomic definition of the second.

There were several novel interfaces out of which the atomic definition of the second emerged. First, there was the interface between the different elements of the clock: the applied radiation, the cesium atoms, and then the traditional quartz crystal clock. It was through the process of coordinating of these elements that the atomic second came into being, and this process continues to define the second. This type of coordination now lies at the heart of a time standard, and has come to define what it means to determine a time measure. Further, there was the interface between the Essen's cesium device and the ephemeris time Markowitz transmitted through radio signals. This process of coordination involved the calibration of the ephemeris and cesium standards to a third time standard, universal time, in order to demonstrate the consistency of the cesium and ephemeris standards. Finally, atomic time brought the Newtonian variable "t", which was presupposed in ephemeris time, together with the variable "t" that was presupposed in the definition of the frequencies established in the cesium device. Thus, the establishment of the atomic second emerged out of a series of co-ordinations at a variety of interfaces, all of these interfaces novel to timekeeping.

The concept of a second, as a standard unit of time measurement, was in flux during the early years of atomic timekeeping for a variety of reasons. The notion of a time standard was destabilized because atomic devices blurred the boundaries between a clock and a standard; further, the relation between time measures and the human experience of change was up for debate. Finally, the coordination process involved in the development of an atomic time standard brought together various understandings of a measure of time at a variety of interfaces. All of this contributed to the novel way in which the atomic second came into being.

4.3 What Is Time?

What does a clock measure? What does a second capture? The concept of time is bound up with clocks and time standards; thus, changes in the ways physicists understood clocks and time standards affected how they understood time in general. The changes that took place in timekeeping during 1940s and 1950s reveal how heterogeneous and multifaceted the concept of time was during this period. During the early days of atomic timekeeping it was common to refer to different types of time, for example astronomical time or physical time, and each type of time brought with it a particular set of meanings and associations. Time was and a heterogeneous concept, used by different people for different purposes. For some it was bound up with the cycles of human experience, while for others it was deeply bound to resonance frequencies. Time did not have a fixed meaning or set of meanings, yet time was nevertheless a foundational concept presupposed in the multiplicity of other timekeeping concepts. What, if anything, held all of these concepts together?

The development of the atomic second brought together two different theoretical understandings of time – the Newtonian variable "t", which was tied to intuitive understandings of motion, including the orbits of the planets, and the variable "t" as it was understood in terms of electromagnetic frequencies. Prior to atomic timekeeping, the more intuitive notion of "t" from Newtonian physics had defined the second; with the atomic definition, a less intuitive "t" took over. And while there was some objection, by those like Markowitz, who felt the "t" associated with planetary motions was the more practical standard, everybody involved, including Markowitz, took for granted that these concepts of "t" were commensurable. Decisions had to be made about how to define the time conventions used by scientists and disseminated internationally; nevertheless, these decisions presupposed a truth of time, underlying all of physical theory and measurement, which was more fundamental than the conventions up for debate. Time was a heterogeneous concept, and multiple understandings of time were at play during this period; nevertheless, the scientists involved upheld a basic presupposition about the truth and commensurability of the multiplicity of "t"'s of physical theory. In chapter two of this dissertation I will look more closely to physicists' presuppositions about time as a physical variable "t". After the present analysis of time as a unit of measure, which reveals the contingencies, values, and agendas at work in the development of atomic timekeeping, I will look to changes in understandings of time as a physical variable, the truth of which was presupposed by the scientists considered in this chapter.

5. Conclusions

The efforts of Markowitz, Lyons, and Essen to develop atomics clock and time standards belonged a new era of atomic time that complicated traditional timekeeping concepts. Neither of these three men, nor others involved in the early days of atomic timekeeping, explicitly asked questions about the meaning of timekeeping concepts, for to do so would have fallen outside of the professional boundaries they set for themselves. Nevertheless, the clocks and standards they developed were different in kind from those that had come before, and fundamental questions about basic concepts were implicitly raised in their work. The meanings of these concepts were at stake in the priority dispute over who invented the first atomic clock, as is revealed through the lack of consensus surrounding which device counted as the first atomic clock. In addition, the question of how to define the second - in terms of the relationship between the astronomical second and the atomic second, as well as the changes in the specific techniques used to establish the atomic second - showed how the concept of the second was by no means fixed. While the physicists and astronomers involved were not concerned with fundamental questions, and did not speculate about the nature of time per se, conceptual changes and multiplicities arose out of their work.

The fact that scientists didn't discuss the nature of time keeping concepts can be partially explained by the pragmatic culture within the institutions that supported atomic clock research during this period, as well as the tone set by influential physicists in the field such as Rabi. The institutions in which these physicists worked fostered a pragmatic approach to physics, due both to government involvement and the wartime legacy of radar research. This was in addition to the continuity between the wartime work of the physicists involved and their work on atomic clocks. The fact that these physicists did not speculate about the nature of time adds contour to the nature of the pragmatic, postwar culture of atomic timekeeping. It shows that these physicists felt that deep probing into the nature of their basic concepts was beside the point of their work, and did not consider speculating about such questions. In chapters two and three, I will contrast this attitude with other communities of postwar physicists who had more expansive visions of the role of the physicist, and allowed some room for philosophical speculation within their profession.

Atomic time came into being within the context of several historical contingencies, particularly the tensions between astronomy and physics and between universality and contingency. Human agendas and values went into the definition of the atomic second, which should not be taken for granted as objectively true. Further, the status of the ideal of the universality of time measures in relation to the contingency of time, as well as the disciplinary role of astronomers in relation to physicists with respect to timekeeping, was at stake during this period. Timekeeping concepts were in flux within the context of these contingencies; nevertheless, the physicists and astronomers involved presupposed that there was a truth about time that conditioned the various devices and standards they built. The time of physics theory – the variable "t", and the connection between this variable, different theories, and time keeping conventions - was taken for granted throughout this period. Chapter two will consider the notion of time as a physics variable *per se*, considering developments that complicated this further layer of the concept of time.

CHAPTER TWO

The Discovery of CP Violation: Physics, Fundamentality, and the Arrow of Time

1. Introduction

In 1964, high-energy particle physicists Val Fitch and James Cronin conducted an experiment for which they won the 1980 Nobel Prize in Physics. In the years following the experiment, both men described insight into the nature of time as one of the reasons their results were significant. The relevance of their experiment to the concept of time pertained to time-reversal symmetry in physics (T-symmetry), a symmetry principle stating that physical laws are not affected by a change in the direction of time. Before Fitch and Cronin's experiment, particle physicists assumed that T-symmetry was universally valid; however, the experiment implied that T-symmetry is violated during certain rare physical processes. According to Fitch and Cronin, the implied violation of T-symmetry offered deep insight into the nature of time, one of the most fundamental concepts in physics. Val Fitch opened his Nobel Prize acceptance speech by stating that the violation of T-symmetry "touches on our understanding of nature at its deepest level."[100] Or, as James Cronin wrote in a 1982 article for *Physics Today*, the violation of

[100] Val Fitch, "The Discovery of Charge-Conjugation Parity Asymmetry," 1980 Nobel Lecture in *Nobel Lectures in Physics: 1971-1980* (New Jersey: World Scientific, 1992).

T-symmetry “matter[s] because it relates to one’s fundamental understanding of space and time.”[101]

The asymmetry of physical systems under time reversal has been a source of discussion among physicists and philosophers since the nineteenth century, primarily with respect to the second law of thermodynamics.[102] An asymmetry under time reversal is built into second law of thermodynamics, which describes the tendency of the entropy of a system to increase in time. At the time of Fitch and Cronin’s experiment, there was a consensus within the scientific community that this asymmetry was attributable to the contingent boundary conditions of a system, and particularly to the low entropy state of the universe at the time of the big bang.[103] However, the asymmetry implied by Fitch and Cronin’s experiment was of a fundamentally different nature than that associated with increasing entropy. Fitch and Cronin’s experiment implied that the direction of time affects physical processes even when the initial and final conditions are reversed. This meant that questions about the direction of time in relation to physical processes, which physicists had generally agreed was resolved, could be reopened.

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[101] James Cronin, “CP Symmetry Violation,” Physics Today (June, 1982): 38.

[102] For an overview of this topic, see for example Huw Price, *Time’s Arrow and Archimedes’ Point* (Oxford: Oxford University Press, 1997); Sean Carroll, *From Eternity to Here: The Quest for the Ultimate Thoery of Time* (New York: Dutton, 2010); or David Albert, *Time and Chance* (Cambridge MA: Harvard University Press, 2000).

[103] This idea was first proposed by Boltzmann. For a discussion of boundary conditions in thermodynamics, see Craig Calendar, “Thermodynamic Asymmetry in Time," *Stanford Encyclopedia of Philosophy*, available at http://plato.stanford.edu/entries/time-thermo/ .

Debates surrounding time reversal symmetry in relation to thermodynamics have involved a wide range of philosophical issues. For example, debates about the nature of time in the nineteenth century, catalyzed by issues surrounding time reversal in thermodynamics, involved questions ranging from determinism to free will to conceptions of God. Further, philosophical thinkers who have discussed time reversal in the context of thermodynamics in the twentieth century have identified connections between time reversal symmetry and philosophical questions about consciousness, experience, and the nature of reality.[104] However, the violation of T-symmetry implied by Fitch and Cronin's experiment has not been the subject of the same types of conversations among physicists and philosophers as the asymmetry associated with thermodynamics. There has been a small amount of philosophical attention to the violation of T-symmetry among analytic philosophers of science, although such discussion has been marginal among larger philosophical debates about the physics of time.[105] Further, questions about how the violation of T-symmetry provides insight into the nature of time, and why such insight is profound, have neither been asked nor answered by the physicists involved in the discovery. In several articles and interviews, Fitch and Cronin have explained what time-reversal asymmetry literally means – ie. that

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[104] See, for example, Hans Reichenbach, *The Direction of Time* (Mineola, New York: Dover Publications, Inc.,1956).

[105] For example, Craig Callendar, "Is Time 'Handed' in a Quantum World?" *Proceedings of the Aristotelian Society*, Vol. 100 (2000): 247.

certain physical process behave differently when the direction of time is reversed.[106] However, they have never offered any reflection on how this asymmetry can provide insight into the concept of time. When asked how their work sheds light on the concept of time, both Fitch and Cronin dismissed the question as "philosophy," a domain with which they had little to do. Fitch responded by saying, "That's a question for a philosopher. I'm not a philosopher. […] It would be somewhat pretentious of me to start thinking such […] thoughts; I'm more of a nuts and bolts person."[107] Along similar lines, Cronin responded, "I wouldn't know how to answer that question; I'm a physicist not a philosopher".[108] While Cronin and Fitch invoked the concept of time to explain why their results were important and profound, they never engaged in any explicit discussion of how T-violation affected the concept of time, a discussion they believed to fall outside the boundaries of their work and expertise.

In this chapter, I will situate Fitch and Cronin's experiment, along with its implications for the concept of time, within the professional boundary between particle physics and philosophy in the postwar United States. Beginning with a brief overview of the experiment and the implied violation of T-symmetry, I will argue that the manner in which Fitch and Cronin discussed the concept of time in relation to their experiment can

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[106] For example, James Cronin, "CP Symmetry Violation," *Physics Today* (June, 1982): 38.

[107] Interview with Val Fitch by the author, March 4, 2011.

[108] Interview with James Cronin by the author, September 24, 2010.

provide insight into how these physicists understood the boundaries of their discipline. I will further argue that these boundaries reflect the way particle physicists understood their professional identities during this period, in particular by illuminating a defining tension between, on the one hand, asking the deepest and most profound questions about the universe, and on the other hand carrying out no-nonsense, practically-minded science. Looking at how Cronin and Fitch conceived of the boundary between physics and philosophy with respect to the question of time, I will shed light on how these physicists understood the nature and purpose of their discipline in relation to philosophical questioning.

Further, in this chapter I will situate the ways in which postwar particle physicists conceptualized time within the theoretical and experimental practices of their communities. Drawing upon works in the history of science that have analyzed these practices – including *Constructing Quarks* by Andrew Pickering, *How Experiments End* and *Image and Logic* by Peter Galison, and *Drawing Theories Apart* by David Kaiser – I will show how variables like time in particle physics were not taken up as objects of deep philosophical investigation. This will explain not only why particle physicists themselves did not investigate the philosophical implications of Fitch and Cronin's result, but also why this result has not yielded much in the way of philosophical insight by other thinkers with more expansive professional identities.

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Building on chapter one of this dissertation, chapter two adds a further dimension to the landscape of professional identities of postwar physicists. While the physicists in chapter one were firmly pragmatic in their orientation toward physics, partially due to the institutional context in which they were working and the example set by prominent physicists within the field, the physicists I consider in chapter two were more open to philosophical insights. Fitch and Cronin's work was technically and empirically-minded regarding physics, and yet it raised profound questions about the concept time; thus, it inadvertently spoke to a more expansive, philosophical discourse about the nature of time. Although not explicitly intended, Fitch and Cronin did not ignore the potential philosophical implications of their work, and used fundamental questions about the nature of time to explain for the significance and importance of their experiment. This being said, Fitch and Cronin never actually engaged in philosophical lines of questioning. In this chapter I will suggest a distinction between, on the one hand, how these physicists justified the importance of their work, and on the other hand, how they approached their work in practice. I will argue that for Fitch and Cronin, the tension between fundamentalism and pragmatism in postwar American physics, outlined in the introduction to this dissertation, took the form of a distinction between justification and practice.[109] That is, Fitch and Cronin saw relevance to deep, philosophical questions as a

[109] For discussions of the tension between philosophy and pragmatism in 20th century American physics, see Silvan S. Schweber, "The Empiricist Temper Regnant: Theoretical Physics in the United States, 1920—1950," *Historical Studies in the Physical and Biological Sciences*, Vol. 17 (1986): 55-98; Alexi Assmus, "The Americanization of Molecular Physics," *Historical Studies in the Physical and Biological Sciences*, Vol. 23, No. 1 (1992):1 – 34; Nancy Cartwright, "Philosophical Problems of Quantum Theory", in *The Probabilistic Revolution, V2*, ed. Lorenz Kruger (Cambridge, MA: MIT Press,

justification for their work, and yet in practice they embodied the pragmatist tradition in which they were trained.

Further, in this chapter I will build on the discussion in chapter one of the contingencies and presuppositions that conditioned the concept of time during the postwar period. In chapter one I looked to the concept of time in the context of timekeeping, showing the values and agendas that went into physicists' understandings of the concepts of "clock", "second", and time", revealing the multiplicity of changing understandings of these concepts. Further, I noted how despite these changing understandings, physicists presupposed that there existed a variable "t", consistent throughout various physical theories, and capable of being captured by clocks. In chapter two I will consider this variable "t" directly, in the context of postwar particle physics, to understand the way it was at stake during this period. In this way, this chapter will contribute to the second line of argumentation that runs through this dissertation, concerning the ways American physicists' concepts of time changed during the postwar period, the multiplicity of contingencies and presuppositions that conditioned these concepts, and the role that fundamental concepts like time played within physics during this period.

In section two of this chapter I will discuss the details of Fitch and Cronin's experiment, along with some of the major concepts in particle physics required to

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1990); Peter Galison, *How Experiments End* (Chicago: University of Chicago Press, 1987); Peter Galison, *Image and Logic* (Chicago: University of Chicago Press, 1997); and David Kaiser, *How the Hippies Saved Physics: Science, Counterculture, and the Quantum Revival* (New York: W.W. Norton and Company, 2011).

understand these details. I believe it is important to work through these details for several reasons. First, they offer a to sense of the type of technical, experimental work Cronin and Fitch were doing, which is important when considering the nature of their mix of philosophy and pragmatism. Second, these details are necessary to understand the initial context and motivations for the experiment, which differed from the way the experiment was discussed and justified in the years that followed.

Following the technical discussion in section two, section three will briefly discuss historical debates surrounding question of time reversal symmetry in physics, in order to understand the relationship between these debates and the T-violation implied by Fitch and Cronin's experiment. Here I will emphasize the continuity and discontinuity of discussions about T-violation in Fitch and Cronin's experiment with historical conversations about time symmetry in physics. In section four I will discuss the perceived significance of the CP violation experiment among particle physicists, particularly in relation to "deep", fundamental questions. Section five looks more deeply to the concept of time as a variable in the context of the CP violation experiment, as understood within the context of theoretical and experimental practices within postwar particle physics. By way of conclusion, I will discuss what this episode reveals about the identity of this subgroup of postwar particle physicists, and how they are situated within the larger context of pre-war and post-war theoretical physics. Further, I will discuss how this episode builds on chapter one to reveal a further dimension to the understandings and assumptions about the nature of the concept time in the postwar period.

2. The Experimental Discovery of CP Violation: Technical Details

2.1 Discrete Symmetries in Particle Physics

Within physics, a symmetry principle requires that the laws of physics be the same before and after a given change to a system. For example, “space translation” symmetry requires that if an experimental apparatus is moved to a different location in space, the results of the experiment will remain the same. Similarly, “time-reversal” symmetry (T-symmetry) requires that the results of an experiment be the same before and after the direction of time is reversed. In addition to T-symmetry, there were two other important symmetry principles relevant to Fitch and Cronin’s 1964 experiment. One of these was “space-inversion symmetry” which requires that an experimental set-up obey the same physical laws as its mirror image. Another was “charge-conjugation” symmetry, which requires that the laws of physics remain the same when matter is replaced with antimatter. Space-inversion symmetry and charge-conjugation symmetry are referred to respectively as “P-symmetry” and “C-symmetry.”[110] Prior to 1956, particle physicists assumed that P-symmetry, C-symmetry, and T-symmetry were universally valid for all physical processes. In addition, the prevailing physics theory at the time required that the combination of all three symmetries – CPT symmetry –be universally valid.[111]

[110] “P” stands for Parity, the quantity that is conserved when space-inversion symmetry is valid. Every symmetry principle in physics is associated with a conserved quantity, according to “Noether’s Theorem”. For a discussion of the relationship between symmetry principles and conservation laws, see David Griffiths, *Introduction to Elementary Particles* (Weinheim: Wiley-VCH, 2008), 116-117.

[111] CPT is a direct implication of quantum field theory, and can be traced to the work of Julian Schwinger, Wolfgang Pauli, and Gerhart Luders. For a detailed discussion of the principles involved, see R. Streater and A. Wightman, *PCT, Spin Statistics, and All that*

The validity of C, P, and T symmetries was first called into question in 1956, when Chen Ning Yang and Tsung Dao Lee pointed out that no experiment had ever directly tested for the validity of P-symmetry.[112] Upon the urgings of Yang and Lee, Chien-Shiung Wu carried out an experiment at Columbia University to test for P-symmetry later that year. The results of the experiment were quickly viewed as definitive from within the particle physics community: under certain conditions P-symmetry is violated, which is to say the mirror image of an experimental set-up does not always yield a mirror image of the results.[113] Following the experimental demonstration of the violation of P-symmetry by Wu, particle physicists explained the effect in terms of properties of the specific particles used in the experiment, referred to as the helicity or handedness of particles.[114] The phenomenon of P-violation was construed as a result of an asymmetry belonging to specific particles; however, the same type of particle-based explanation was not possible in the case of Fitch and Cronin's discovery of CP violation.[115]

(New York: W.A. Benjamin, 1964); or S. Schweber, *An Introduction to Relativistic Quantum Field Theory* (Evanston: Row, Peterson, 1961).

[112] As described in Chen Ying Yang, "The Law of Parity Conservation and Other Symmetry Laws of Physics," 1957 Nobel Lecture in *Nobel Lectures in Physics: 1942-1962*, (New Jersey: World Scientific, 1998).

[113] C.S. Wu et al. "Experimental Test of Parity Conservation in Beta Decay," *Physical Review*, **105**, (1957): 1413; For an overview of Wu's experiment, see Allan Franklin, *The Neglect of Experiment* (Cambridge: Cambridge University Press, 1989), 7-72.

[114] Helicity is can be determined by the relation between direction of motion to the spin of a particle. See Griffiths, *Introduction to Elementary* Particles, 136-142.

[115] See Franklin, *The Neglect of Experiment*.

In response to Wu's discovery of P-violation, Murray Gell-Mann and Abraham Pais proposed that while P-symmetry may not always hold by itself, the combination of P-symmetry and C-symmetry – CP symmetry - is never violated.[116] Through the proposal of the invariance of CP symmetry, Gell-Mann and Pais accomplished several things. First, they created a way to preserve the underlying principles of a theory they had proposed in 1955, regarding the behavior of certain particle systems under the weak interaction.[117] Their theory had been founded on the assumption of P-symmetry invariance; however, they were able to show how the invariance of CP symmetry would be able to preserve the structure of these ideas.[118] Second, in combination with the requirement that CPT symmetry always be valid, the universal validity of CP symmetry was able to preserve the universal validity of T-symmetry. That is, if CPT symmetry is always valid, then the invariance CP symmetry implies the validity of T-symmetry. Conversely, if CP were to be violated, then T-symmetry would necessarily be violated as well. Gell-Mann and Pais believed the invariance of T-symmetry to be a fundamental principle; therefore, by proposing CP symmetry invariance, this principle would be preserved. In the early 1960s, when Fitch and Cronin conducted their famous experiment,

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[116] As explained in T. D. Lee, R. Oehme, and C. N. Yang, *Phys. Rev.* **106** (1957): 340.

[117] M. Gell-Mann and A Pais, *Physics Review*, **97**, (1955): 1387. A description of Gell-Mann and Pais' original theory can be found in James Cronin, "The Experimental Discovery of CP Violation," Nishina Memorial Lecture, in *Lect. Notes Phys.* **746** (2008): 261–280.

[118] For a detailed explanation of how Gell-Mann and Pais used CP invariance to their explanation of their theory and predictions, see Cronin, "The Experimental Discovery of CP Violation," 263.

CP, T, and CPT were all thought to be universally valid symmetries. Physicists accepted that P-symmetry was occasionally violated, and thus C-symmetry was violated in the same instances; however, these symmetry violations were attributed to the helicities of specific particles.

2.2 The Experiment

In 1963, both Val Fitch and James Cronin were Princeton professors, conducting experiments at the Brookhaven National Physics Laboratory in Upton, New York.[119] James Cronin had received his PhD from the University of Chicago in 1955, and soon after had joined a group of Princeton physicists working at a new Brookhaven particle accelerator, known as the Cosmotron.[120] During WWII Fitch had worked on the Manhattan project for three years, before finishing an undergraduate degree at McGill University. He went on to complete his Ph.D. in Physics at Columbia University in 1954. He took a position at Princeton late 1954, which brought him to Brookhaven. During the late 1950s and early 1960s, Fitch and Cronin got know each other at Brookhaven, often discussing physics over lunch in the Brookhaven cafeteria.[121]

In 1963, Cronin and Fitch began to discuss the possibility of conducting an experiment to search for an anomaly that another group of physicists had reported,

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[119] For more detail on the history of the Brookhaven laboratory, see Robert Crease, *Making Physics: A Biography of Brookhaven National Laboratory, 1946-1972* (Chicago: University of Chicago Press, 1999).

[120] James Cronin, "Autobiography," Nobelprize.org .

[121] Val Fitch, "Autobiography," Nobelprize.org .

involving particles called neutral kaons. Fitch and Cronin's primary motivation for the experiment was to test for this anomaly, which had been detected in a newly established effect called "regeneration."[122] Cronin had an experimental set-up that would be able to test for this anomaly, with which he was already working with at Brookhaven. This apparatus involved spark chambers detectors that he built himself. Fitch was interested in the anomalous effect due to its potential relevance to a set of experiments he had been conducting at Brookhaven.[123] Fitch and Cronin's secondary objective in conducting the experiment was to test what they referred to as "CP invariance", and determine the extent to which CP symmetry was known to be valid to a greater degree of accuracy. Their third and final objective was look for the presence or absence of "neutral currents" in their apparatus. As stated in their proposal for funding for the experiment:

> The Present Proposal was largely stimulated by the recent anomalous results of Adair et al, on the coherent regeneration of the K10 mesons. It is the purpose of this experiment to check these results with a precision far transcending that attained in the previous experiment. Other results to be obtained will be a new and much better limit for [CP invariance] [and] a new limit for the presence (or absence) of neutral currents […].[124]

In their proposal, Fitch and Cronin described the part of their experiment as that dealt with CP symmetry as involving CP "invariance", although the experiment is now always

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[122] The anomalous effect was published in L. B. Leipuner, W Chinowsky, R. Crittenden, R. Adair, B Musgrave, and F. T. Shively, *Phys. Rev.* 132 (1963): 2285.

[123] James Cronin, "The Experimental Discovery of CP Violation," 265.

[124] J. W. Cronin, V.L. Fitch, R. Turlay, "Proposal for K20 Decay and Interaction Experiment," April 10, 1963. As printed in Appendix to: Cronin, "The Experimental Discovery of CP Violation".

referred to as the discovery of CP "violation".[125] According to their accounts, they had no expectation that they would find evidence of CP symmetry violation.[126] As a secondary objective, they hoped to use the apparatus, already running to find the anomaly, to test the certainty with which CP symmetry invariance was known to be valid. At the time, CP invariance was established to an accuracy of two decimal points; Fitch and Cronin intended to use their experimental set up to increase this accuracy to three decimal points.[127]

Fitch and Cronin's experiment involved the production of neutral kaons in a particle accelerator, and the analysis of the decay products. Neutral kaons exist in two states, one with a lifetime on the order of 10^{-10} seconds, and another with a longer lifetime of 10^{-8} seconds.[128] If CP symmetry obtains, kaons in the shorter-lived state will always decay to two pions, while kaons in the longer-lived state will decay to three pions. The experimental set-up involved a beam of neutral kaons that were allowed to decay in the apparatus below. By the time the kaons entered the apparatus, all of the short-lived

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[125] *Ibid.*

[126] See, for example, Cronin, "The Experimental Discovery of CP Violation".

[127] Technically the accuracy is referred to as the "upper limit" of the branching ratio for the decay of kaons into pions. The upper limit previous known was 1/300 (see D. Neagu et. al, "Decay Properties of $\mathbf{K_2^0}$ **Mesons"** *Physical Review Letters,* B (1961): 552-553). Fitch and Cronin were hoping to extend the upper limit to greater than 1/1000.

[128] The idea that there are two different neutral kaon states, with different lifetimes, behaving behave differently under the weak interaction, and superpositions of the particles acted on by the stronger interaction, was proposed by Pais and Gell Mann in their famous paper, "Behavior of Neutral Particles under Charge Conjugation" *Phys. Rev*. **97** (1955): 1387–1389.

kaons had decayed. The long-lived kaons decayed in a helium bag, and the decay products were tracked in Cronin's spark chambers, which were located in the arms of the apparatus.[129] Fitch and Cronin recorded their data in a series of notebooks. Figure 2.3 below shows the first page of data entry for the portion of the experiment devoted to CP-symmetry. Note that the section is entitled "CP invariance" as opposed to "CP violation".[130]

The experiment did not immediately run smoothly. As Cronin described in later accounts of the experimental process, "This was not a smooth run – it was the real world!"[131] The notebooks that Cronin and Fitch used to record their data over the course of the experiment chart many of the setbacks Fitch and Cronin encountered.[132] These included technical errors that rendered entire sets of data meaningless, external effects unaccounted for that invalidated results, and one occasion during which their apparatus was struck by lightening. There were also several instances of human error on the part of the various technicians monitoring the experiment, described by Cronin or Fitch in their notebook entries.[133]

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[129] *Evidence for the 2π Decay of the* K_2^0 *Meson," Physical Review Letters* ***13*** *(1964): pp. 138-140;* and James Cronin, *The Experimental Discovery of CP Violation,* Lect. Notes Phys. **746,** 261-280 (2008). See figure 2.1 for schematic diagram of the apparatus, and figure 2.2 for the only surviving photograph of the apparatus.

[130] Val Fitch and James Cronin, *Laboratory Notebooks*, 1963, from personal collection of Val Fitch.

[131] Cronin, "The Experimental Discovery of CP Violation".

[132] Fitch and Cronin, *Laboratory Notebooks*.

[133] *Ibid.*

After running their experiment for several weeks, and spending several more analyzing the results, Fitch and Cronin observed that approximately 1/500 neutral kaons in the longer-lived state decay to two pions, as opposed to the expected three pions.[134] This was determined by measuring the pion momentum, using the principles of conservation of mass and conservation of momentum to determine whether the decay had resulted in two charged pions or two charged and one neutral pion. Finally, Fitch and Cronin compared their results to Monte Carlo simulations of the expected results, if 3-pion decay were not admitted. Their results shows that around 1/500 kaons had decayed to 3 pions, which was a statistically relevant.[135] This implied that CP symmetry was occasionally violated, and due to CPT theorem, T-symmetry must be violated in these instances as well.

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[134] *Ibid.*

[135] See figures 2.4, 2.5, and 2.6; also see *Evidence for the 2π Decay of the* K_2^0 *Meson," Physical Review Letters **13** (1964): pp. 138-140.* For a more detailed description of the results, see James Cronin, "The Experimental Discovery of CP Violation".

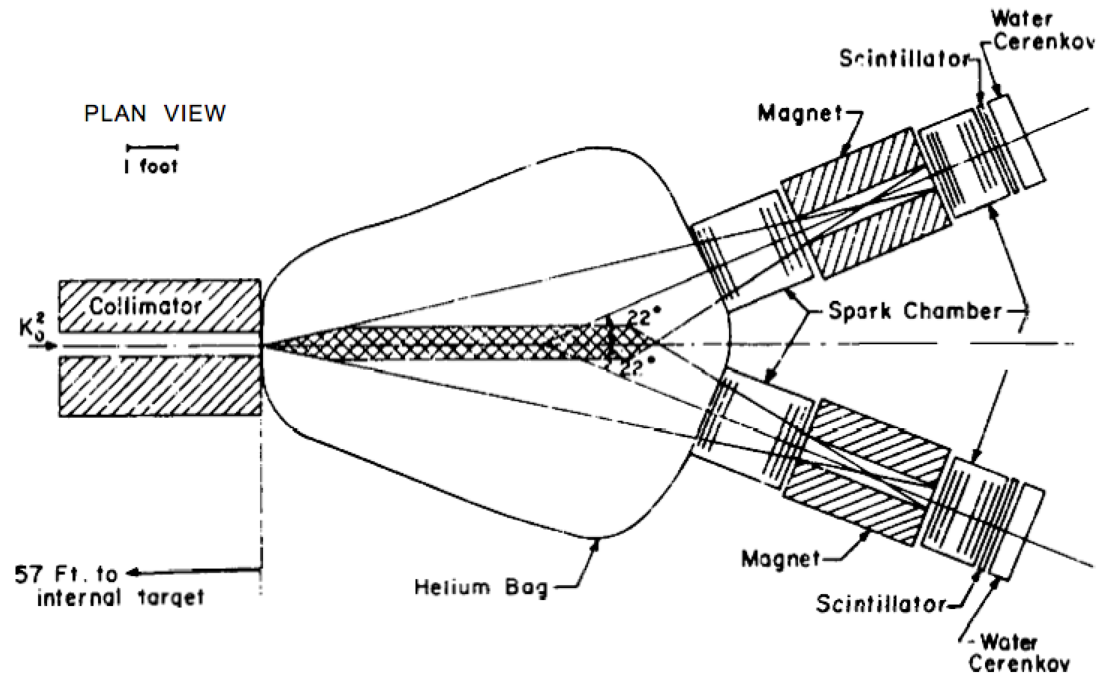

Figure 2.1: Schematic Diagram of Apparatus, taken from James Cronin et. al., "Evidence for the 2π Decay of the K_2^0 Meson," Physical Review Letters 13 (1964): pp. 138-140.

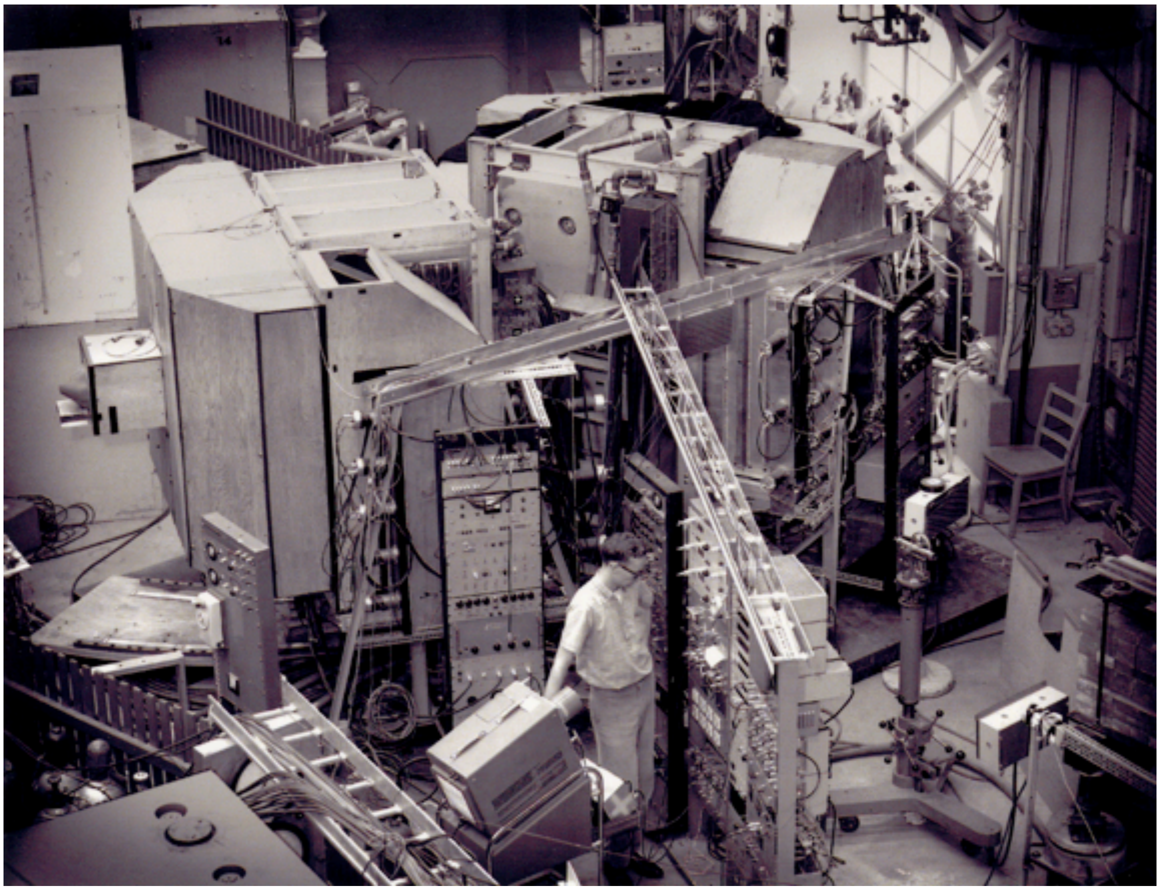

Figure 2.2: Photograph of a portion of the apparatus used in the discovery of CP violation

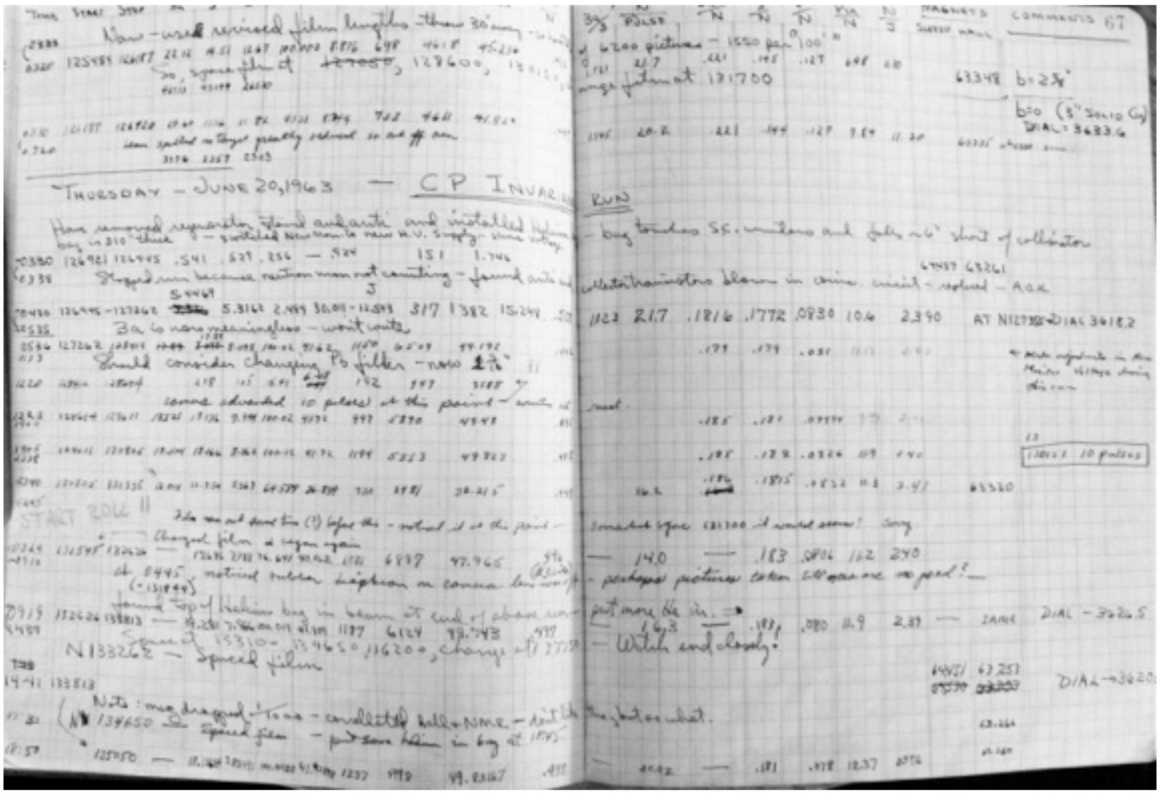

Figure 2.3: Page from Fitch and Cronin's notebooks, from the portion of the experiment devoted to testing CP "Invariance".

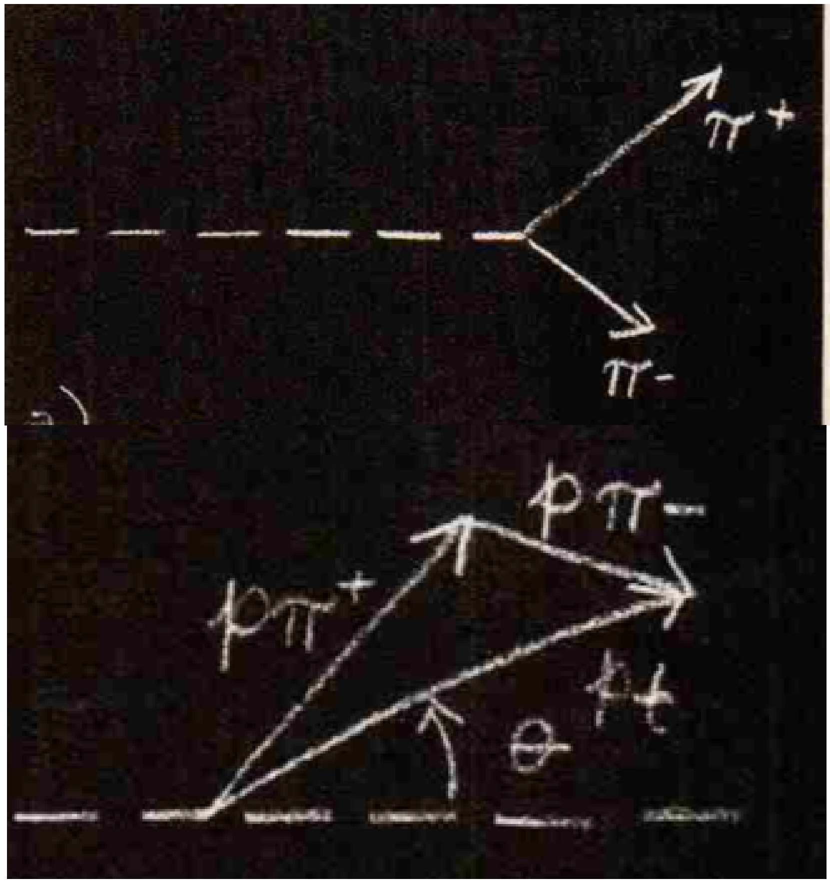

Figure 2.4: Drawings by James Cronin to explain calculations used to determine CP invariance/violation. If the angles of the momenta of the two observed pions added to zero, this suggested a 2 pion decay. If the angles did not add to zero, it suggested a 3 pion decay.

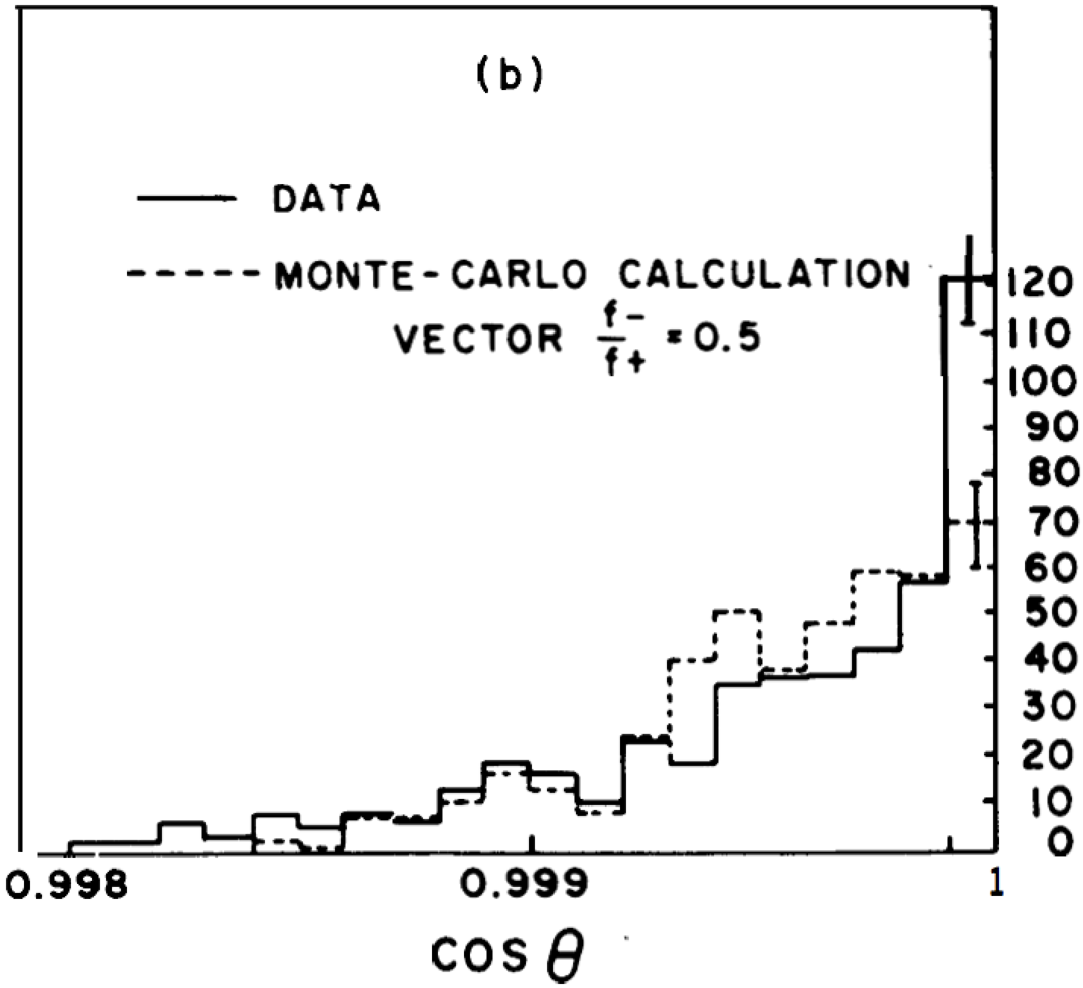

Figure 2.5: Number of Events Versus Cosθ, Compared to Monte Carlo Calculation

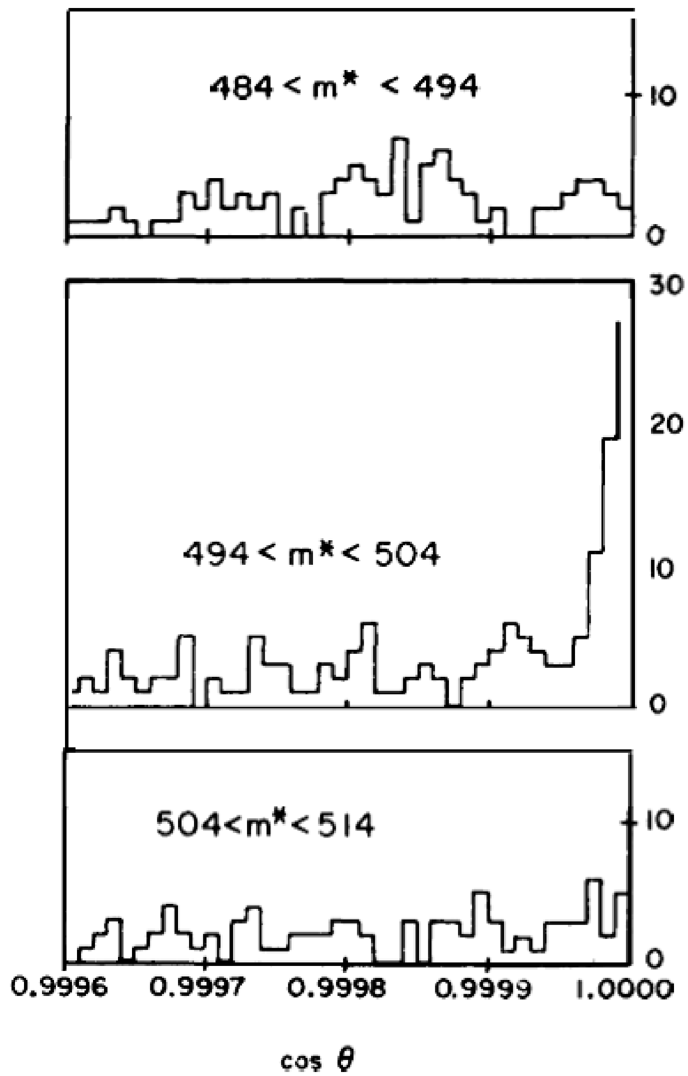

Figure 2.6: Number of events versus Cosθ for different parent masses.

2.3 Reception

Fitch and Cronin's results were published in *Physics Review Letters* a few months after they carried out their analysis showing evidence of CP violation.[136] The results attracted a great deal of attention from within the particle physics community. CP violation was not immediately accepted, and several groups proposed alternate explanations for the findings, as well as attempted to replicate the experiment.[137] In his Nobel lecture, James Cronin recalls that

> Upon learning of the discovery in 1964, the natural reaction of our colleagues was to ask what was wrong with the experiment. Or, if they were convinced of the correctness of the measurements, they asked how could the effect be explained while still retaining CP symmetry. I remember vividly a special session organized at the 1964 International Conference on High Energy Physics at Dubna in the Soviet Union. There, for an afternoon, I had to defend our experiment before a large group of physicists who wanted to know every detail of the experiment.[138]

After several different groups replicated the results, and eventually all of the alternate theories were dismissed, a consensus was reached in the physics community that CP symmetry, and indirectly T symmetry, is occasionally been violated.[139]

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[136] *Ibid.*

[137] For example, A. Abashian et al, *Physics Review Letters,* **13** (1964): p. 243.

[138] James Cronin, "CP Symmetry Violation – The Search of Its Origin".

[139] For a review of replications, see A. Abashian et. al, "Search for CP nonconservation in K_2^0 decays," *PRL*, 13 (1964), 143-146. For a description of how consensus was achieved, See Allan Fraklin, "The Discovery and Acceptance of CP Violation," *Historical Studies in the Physical Sciences,* **13** (1983): 207-238.

After it was agreed upon that CP violation occasionally occurs in the decay of neutral kaons, the question remained whether CP violation occurs in other systems as well. Physicists at several labs began searching for CP violation in systems other than the neutral kaon system, for example in the decay of B-mesons under the weak interaction. CP violation was eventually observed in the B-meson system in experiments conducted with accelerators in both the United States and Japan in 2001.[140] At the present time, all evidence of CP violation has involved the decay of particles involving the weak interaction, and it is still an open question whether CP symmetry obtains under other forces, particularly the strong force.[141]

In addition to the implied T-violation, there have been other intriguing implications of CP violation discovered over the years – for example the possibility that CP violation could explain the matter/antimatter asymmetry in the universe.[142] The possibility that CP violation could be used to explain matter/anti-matter asymmetry was first pointed out by Andrei Sarakhov in 1967; however, it was not taken seriously by the physics community until the 1980s when the field of cosmology, and questions about matter, anti-matter, and the big bang, became a major field of physics research. [143] Yet, as

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[140] A. Abashian et al. *Physical Review Letters,* **86** (2001): 2509; B. Aubert et. al, *Physical Review Letters,* **86 (**2001): p. 2515..

[141] See Griffiths, *Introduction to Elementary Particles*, 148-149.

[142] F. Wilczek, *Scientific American*, (December 1980): p. 82.

[143] A.D. Sakharov, "Violation of CP Invariance, C asymmetry, and Baryon Asymmetry of the Universe," in *Journal of Experimental Theoretical Physics* 5 (1967): 24-27; for a description of the rise of particle cosmology in the 1980s, see David Kaiser, "Whose

we shall see, at the time Fitch and Cronin's experiment was carried out and during the immediate years afterward, Fitch and Cronin referred to bearing on the concept of time as one of the major sources of their discovery's significance.

3. Time Reversal Asymmetry

What does it mean for T-symmetry to be violated? The idea of time reversal symmetry in physics is a technical concept. It has nothing to do with travelling back in time; nor does it refer to the subjective experience of time.[144] Time symmetry in physics requires that the laws of physics take the same form when the direction of time is positive as opposed to negative. Consider an analogy often used to illustrate this point: imagine a film recording of a physical system, played first in the forward direction and then in reverse. If this system is "time-symmetric," then the physical laws governing the system will be the same in the forward and backward versions of the film – the objects in the film would all obey the same laws in both directions. If the system is not time symmetric, then the film in reverse would display a system behaving according to a different set of laws. In a system that is asymmetric with respect to time, the form of the laws governing a physical system could be used to determine whether the film was being run forward or backward.

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mass is it anyway? Particle cosmology and the objects of theory," *Social Studies of Science* 36 (August 2006): 533-564.

[144] The fact that human beings experience an asymmetry with respect to time, insofar as the 'remember' the past and 'expect' the future, is beyond the scope of the technical time asymmetry, to which CP violation is relevant.

In a time symmetric system, the form of the laws of physics would not be able to distinguish between the two scenarios.

Time reversal symmetry in physics was a controversial issue in the nineteenth century field thermodynamics, particularly involving the concept of entropy and the second law of thermodynamics. The second law of thermodynamics is not symmetrical under time reversal, insofar as it describes the way in which entropy tends to increase in the positive time direction. The time asymmetry of thermodynamics means that one could determine whether or not a film of a physical system was running forward or backward by observing whether the system progressed to a state of greater or lesser entropy. This asymmetry did not apply to Newton's laws of motion, which would not change form when the film was played backward in the backward direction. The discrepancy between the time asymmetry of thermodynamics and the symmetry of Newtonian physics was a serious cause of concern for many nineteenth century thinkers interested in the nature of time in physics.

The significance of thermodynamics, and its relationship to the concept of time reversal symmetry, was been the subject of lively debate among physicists and philosophers alike, in both the nineteenth and twentieth centuries. Much of the focus of these debates in the nineteenth century was on how to interpret the apparent arrow of time built into the second law of thermodynamics, as well as how to reconcile this with the seeming time reversibility of laws of mechanics. As mentioned in the introduction, these debates also touched upon questions about determinism, free will, consciousness, and the nature of reality. However, by the 20th century there was a strong consensus within the physics community that the microscopic laws governing the motion of individual particles *are* time symmetric, and that the apparent time asymmetry of macroscopic thermodynamic laws are a product of the statistical behavior of particles at the microscopic level, in combination with specific boundary conditions.[145] T-violation in the kaon system, as indirectly implied by CP violation in 1964, was unique in that it appeared to be a product of physical laws, and not contingent boundary conditions. Nevertheless, there have not been the same type of debates about the nature of time surrounding the T-symmetry violation implied by CP violation as there have been for the time-asymmetry of thermodynamics. Why did the conversation surrounding CP violation and time take the form that it did?

4. Perceived Significance of CP violation

Physicists have described the discovery of CP violation, which won Fitch and Cronin the 1980 Nobel Prize in physics, as an important and groundbreaking result for a variety of reasons. One such reason has been the relevance of the discovery for insight into the nature of time, particularly relating to time reversal symmetry, although there have been many others as well. In most cases, however, the relevance of CP violation has been explained in terms of a deep insight into nature at the fundamental level. In sections 4.1 and 4.2 below, I will describe some of the ways in which physicists have explained the

[145] For more detail on the role of boundary conditions for the thermodynamics arrow of time see Hans Reichenbach, *The Direction of Time.*

importance of CP violation, with the aim of showing that this group of physicists understood profound, fundamental questions about nature to be vital to their work. I will then use this fact to situate the attitudes of postwar particle physicists within the spectrum of orientations toward the professional boundaries of physics. I will show how they saw their work as relevant to deep, philosophical questions about nature at a fundamental level, even though direct speculations about such questions was outside of their notions of what it meant to practice of physics.

4.1 Time Asymmetry

As described above, relevance to fundamental notions of space and time was one of the primary explanations Fitch and Cronin gave for the importance of their famous experiment. This relevance sprang from the implications of their results for time-reversal symmetry as a basic principle. While they did not explore the implications of their result for understandings of the directionality of physical laws with respect to time, the fact that their results impacted the concept of time was enough to justify its relevance. For Fitch and Cronin, CP violation was significant because it concerned time, and time is a fundamental concept. As Fitch put it, "[…] showing as it does a lack of charge-conjugation parity symmetry and, correspondingly, a violation of time-reversal invariance, it touches on our understanding of nature at its deepest level."[146] Or, as similarly expressed by James Cronin, "Well, you might shrug your shoulders and say, "OK, it's a little anomaly and doesn't matter." But it does matter because it relates to

[146] Fitch, "The Discovery of Charge-Conjugation Parity Asymmetry".

one's fundamental understanding of space and time."[147] Both Fitch and Cronin understood insight into the nature of time to be of the utmost significance for physics. While these physicists didn't explore this implication in any concrete way, Val Fitch elaborated slightly further:

> But for the first time we have in the neutral K mesons a physical system that behaves asymmetrically in time as a result of an interaction, not a boundary condition. […] Since the microscopic physical laws had always been thought to be invariant under time reversal, this discovery opens up a wide range of profound questions.[148]

Cronin and Fitch never addressed these "profound questions". Further, they never formulated the questions in a well-defined way. Unlike in quantum mechanics, for example, where interpretative questions have taken well-defined forms such as the measurement problem, or else in discussions of time-asymmetry in the context of thermodynamics, CP violation never led to well-defined interpretive questions. The physicists involved in the discovery of CP violation described T-violation as raising deep questions in general, without articulating any specific questions along these lines.

How can we explain the fact that Cronin and Fitch saw deep, fundamental questions about basic concepts such as time as one of the reasons their experiment was so important, and yet didn't formulate, ask, or answer any questions that would access this deep fundamentality? Fitch and Cronin's concern with fundamental questions, like those concerning the nature of time, arose in continuity with European physics traditions from

[147] James Cronin, "CP Symmetry Violation".

[148] Fitch, "The Discovery of Charge-Conjugation Parity Asymmetry"

the earlier twentieth century, which saw philosophical investigation into the nature of basic concepts as central to the project of physics. However, the postwar, pragmatic, engineering influenced culture of physics didn't have much space for such speculation.[149] The justification of the CP violation experiment, in terms of its impact on fundamental concepts like time, was tied to an impulse connected to a previous subculture of physicists, that didn't perfectly align with the pragmatist, experimental culture in which Fitch and Cronin were largely immersed. In the introduction to this dissertation, I described postwar American physics as caught between the competing pulls of philosophy and pragmatism. While the physicists described in chapter one were firmly at the pragmatic end of the spectrum, Cronin and Fitch justified their experiments in a way that had much in common with the philosophical sensibility, while remaining thoroughly pragmatic in practice.

However, even if Fitch and Cronin had been interested in speculating about the philosophical implications of their results for the concept of time, they would have had a difficult time knowing where to start. As I will discuss in section five below, the theoretical and experimental practices of postwar particle physics, and the specific ways in which a variable such as "t" was conceptualized within this structure, did not lend themselves to philosophical investigations into the nature of fundamental concepts. Together with the fact that philosophical questioning was not part of Fitch and Cronin's understanding of professional boundaries of experimental physics, this lack of conceptual

[149] For description of philosophical sensibilities among early twentieth century European philosophers, as well as the pragmatic character of postwar American physics, see Kaiser, *How the Hippies Saved Physics*, xiii.

resources to ask profound questions about the nature of time in the context of particle physics meant that Fitch and Cronin only pointed to the profundity of their results, without articulating or exploring it.

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Many twentieth century physicists, philosophers, and popular science writers have written about time reversal symmetry in physics. In general, the thinkers and writers who have discussed this topic in the decades following Fitch and Cronin's discovery have been dismissive the T-violation implied by CP violation, describing it as exotic and irrelevant. For example, physicist Paul Davies' 1974 popular book *The Physics of Time Asymmetry* in 1974 is often cited in technical material on CP violation as an authority and reference point on time reversal in physics.[150] Davies, a theoretical physicist by training, has written many popular books on physics, particularly on the physics of time; *The Physics of Time Asymmetry* was his first such book. In this book, Davies discusses issues surrounding time symmetry and asymmetry in thermodynamics, statistical mechanics, electromagnetism, cosmology, and quantum mechanics. In the section on quantum mechanics, he has a small, two-page section on T-violation, as implied by CP violation. In this section he concludes that although a T-violating law of physics is of the greatest

[150] P. C. W. Davies, *The Physics of Time Asymmetry* (Berkeley: University of California Press, 1974). Davies is cited as an authority on time reversal in physics in, for example, several of the papers included in *Proceedings of the Blois Conference on CP Violation in Particle Physics And Astrophysics,* ed. J. Tran Thanh Van (Gif-sur-Yvette, France: Editions Frontières, 1990).

significance from a fundamental point of view, it is not clear that the properties of K mesons really have any relevance to the type of asymmetric processes that have been under discussion in this book [thermodynamics, etc.]."[151] Davies then moves on to other topics. Like Fitch and Cronin, Davies recognizes the potential significance of T-violation for fundamental understandings of time and the laws of physics; however, also like Cronin and Fitch, he merely notes this, without articulating any questions or ideas about the nature of this significance.

Davies statement about T-violation is typical of the physicists, philosophers and popularizers who have mentioned it in relation to discussions about the arrow of time. If they do mention CP violation, they generally dismiss it as obscure or irrelevant to the questions at hand, despite its potential fundamental importance. For example, popular science writer Brian Green devotes an entire chapter of his book *The Fabric of the Cosmos* to time-reversal symmetry; however, he confines his discussion to thermodynamics and cosmology, mentioning particle physics and T-violation only in a dismissive footnote:

> There are examples, involving relatively esoteric particles, which show that the so-called weak nuclear force does not treat past and future fully symmetrically. However, in my view and that of many others who have thought about it, since these particles play essentially no role in determining the properties of everyday material objects, they are unlikely to be important in explaining puzzle of time's arrow (although, I hasten to add, no one knows this for sure.)[152]

[151] Davies, *The Physics of Time Asymmetry*, 176.

[152] Brian Greene, *The Fabric of the Cosmos* (New York: Vintage Books, 2004), 495.

As will be discussed in section five below, the framework of postwar particle physics did not provide an apparatus or mechanism by which thinkers could ask or answer questions about nature of time raised by implied T-violation; nevertheless, when confronted with T-violation, they gestured to the possibility of its profound significance for understanding the direction of time in relation to physical laws. The work of particle physics involved such narrow, technical concepts that it was difficult for these ideas to be picked up in larger discourses about the nature of time. Popular works exploring philosophical issues surrounding time and time symmetry, such as the above-mentioned works by Davies and Greene, acknowledged that T-violation technically went against currently held ideas about the arrow of time. Nevertheless, these thinkers had no idea how to approach the question of how T-violation challenged the concept of time and its directionality.

4.2 Other Sources of Significance

Many other reasons have been given for the significance of CP symmetry, in addition to its relevance for the concept of time, all of which point to a notion of fundamentality. Each of these reasons provides further evidence that particle physicists during this period believed fundamental questions about basic concepts to be central to their work at the level of justification, while regarding engagement with these questions as beyond the professional scope of physics.

One reason why particle physicists believed Fitch and Cronin's CP violation experiment to be of great importance was its relevance to symmetry principles, which at the time were considered to be among the most fundamental principles in particle physics. Wu's experiment, and the discovery of the violation of parity, set the stage for an understanding of the discovery CP violation as an "overthrowing" of a fundamental principle. Wu's experiment energized the particle physics community for this reason, and the discovery of CP violation occurred in the wake of this energy. The physicists involved in these developments saw this period, during which fundamental principles were being challenged and reconceptualized, as an extremely exciting time for their field.[153] The postwar particle physics community viewed insight into a fundamental concept as being of the utmost importance, revealing that a deep probing of fundamental concepts and principles fell firmly within this community's understandings of the purpose of physics. Even though physicists like Fitch and Cronin were for the most part pragmatic experimentalists, they regarded the probing of fundamental properties of the universe to be one of the higher tasks of their profession.

Further, the particle physics community regarded the violation of CP symmetry as important because the effect couldn't be explained. CP violation went against a fundamental tenet of theoretical particle physics, and could not be easily explained by its theoretical apparatus. This opened up many new avenues of research, motivating new, more fundamental theoretical developments. As mentioned earlier, CP violation could not be explained in terms of a simple attribution of a physical property, like helicity, to the

[153] This sense of excitement is described, for example, in R. K. Adair, "CP Non Conservation - The Early Experiments," in *Proceedings of the Blois Conference on CP Violation in Particle Physics And Astrophysics,* ed. J. Tran Thanh Van, (1990): 37; A. Pais,"CP Violation- The First 25 Years", *ibid,* 3; and Allan Franklin, "The Discovery and Acceptance of CP violation" in *Historical Studies in the Physical Sciences* **13** (1983): 207-238.

particles involved; the explanation would have to go much deeper than the properties of particles.[154] For example, physicists have proposed altering the standard model of physics in order to explain CP violation.[155] The fact that CP violation required a more fundamental explanation was itself a reason why physicists described it as an important result. This explanation of the significance of CP violation is interesting for several reasons. First, it again shows a concern with fundamentality in terms of the way physicists justified the importance of the experiment. Further, it provides insight into what was considered to be a fundamental explanation within this community, as well as which effects count as fundamental and which required a further, more fundamental explanation.

As briefly mentioned above in section 2.3, another justification physicists gave for the importance of CP Violation lay with the fact that it has been used to explain the asymmetry in the universe between matter and antimatter. The possibility of using CP violation to explain this asymmetry was first noted by Andrei Sakharov, a few years after Fitch and Cronin's experiment; however, the idea didn't gain much traction until the 1980s, with the rise of cosmology as a major field of physics research.[156] In this line of

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[154] Griffiths, *Introduction to Elementary Particles*, 148.

[155] *Ibid.,* 409-410.

[156] For original suggestion, see A. D. Sakharov, "Violation of CP Invariance, C asymmetry, and Baryon Asymmetry of the Universe," in *Journal of Experimental Theoretical Physics* 5 (1967): 24-27. For an overview of more recent research in the connection between CP violation and matter/antimatter asymmetry, see John Ellis, "Why Does CP Violation Matter to the Universe?" in *Cern Courier*, 1999, available at http://cerncourier.com/cws/article/cern/28092.

justification, CP Violation is treated as a fundamental principle that explains a derivative effect, as opposed to an effect that requires a more fundamental explanation. This displays interest in fundamentality on behalf of particle physicists once again; however, here CP violation is taken as a fundamental explanation of another phenomenon. The potential to explain matter-antimatter asymmetry is now one of the major reasons both Val Fitch and James Cronin have claimed their result was significant, although this potential did not appear in their initial discussions of the experiment immediately following their discovery.[157]

In each case made for the justification of Fitch and Cronin's experiment, including but not limited to the significance for the concept of time, physicists have appealed to profound, fundamental stakes. However, physicists such as Fitch and Cronin saw themselves as "workbench" physicists, who did not deal with speculative, philosophical ideas.[158] This being said, their concern with fundamentality, and the particular way it played out in the justification of the significance of CP violation, betrays a certain form of philosophical sensibility. This combination of this concern with fundamentality, independent of application, with their version pragmatism, provides insight into the complex contours of the ways in which these physicists understood the meaning and purpose of their work.

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[157] For example, Cronin explained for the significance of CP violation in terms of its relevance to matter/antimatter asymmetry in, "The Experimental Discovery of CP Violation."

[158] Interview with Val Fitch by the author, March 4, 2011.

5. Time as a fundamental concept in postwar particle physics

In this section I will consider the concept of time at stake in Fitch and Cronin's experiment, drawing on a body of scholarship in the history of science that considers the nature of theoretical entities in postwar particle physics. I will explore this scholarship with the aim of of gaining a stronger grasp on the assumptions about time built into Fitch and Cronin's experiment. I will then use this to consider the reasons why Fitch, Cronin, and others used insight into the nature of time to justify the significance of the CP violation experiment, but did not engage in any concrete articulation about the specific possibilities for this type of insight. I will conclude that the concept of time in postwar American particle physics was a complex, multifaceted, and highly technical entity that was not easily transportable into other discourses more accommodating of philosophical investigation. Further, there was no mechanism or interface for which time in the context of Fitch and Cronin's experiment could be brought into conversation with other fields of physics, in which philosophical questions about time reversal symmetry have been discussed. This made it difficult to formulate philosophical questions about the implications of Fitch and Cronin's result for the concept of time.

In his 1984 book *Constructing Quarks*, Andrew Pickering discusses the nature of theoretical entities in postwar particle physics, focusing his attention on the theoretical construction of the quark. Pickering describes theoretical entities as largely the product of judgments on the part of scientists, emphasizing that they are not passive elements of nature as many scientists take them to be. As he writes, "Theoretical entities like quarks […] are in the first instance theoretical constructs: they appear as terms in theories elaborated by scientists."[159] More specifically, he situates these constructs at the interface of theory and experiment, describing them as mediating between the two. It is only after this mediation takes place that scientists come to view them as passive elements of reality. He claims that "theoretical constructs […] serve to mediate the symbiosis of theoretical and experimental practice (and hence to make realist discourse retrospectively possible)."[160] Can time in the context of particle physics be understood as the type of theoretical construct described by Pickering, gaining the appearance of reality at the interface of theory and experiment? The concept of time at stake in Fitch and Cronin's experiment was part of the theoretical framework of quantum field theory, which was presupposed in the construction of the experiment. The experimental run and subsequent analysis then complicated the concept time in this theoretical context, questioning but also reinforcing its meaning. However, a description of time in Fitch and Cronin's experiment in Pickering's terms would be overly simplistic. Several different presuppositions about time were built into the experiment at multiple levels, and there was not one concept of time that could perform this mediating function. Time was the background against which the experiment unfolded, a variable in the equations that described the particle system, a relationship between boundary conditions, and the basis of the time reversal symmetry principle. Taking all of these presuppositions together, the

[159] Pickering, *Constructing Quarks: A Sociological History of Particle Physics* (Chicago: University of Chicago Press, 1984), 7.

[160] *Ibid.*, 14.

concept of time, at the interface of theory and experiment in Fitch and Cronin's experiment, becomes difficult to delineate.

In his 1987 *How Experiments End* and 1997 *Image and Logic*, Peter Galison complicates Pickering's view of the theoretical entities of particle physics by arguing that a variety of material, mathematical, and technical constraints affect the way scientists come to understand theoretical entities. These constraints, in addition to the judgments and decisions of physicists described by Pickering, produce the objects of theory. As Galison writes in direct reference to Pickering's position, "mathematical and physical constraints are not easily brushed aside."[161] Galison describes changes in particle physics as occurring at the non-uniform interfaces of the subcultures of theory, experiment, and instrumentation; further, he details the way shared understandings have been forged out at these heterogeneous interfaces. The case of the discovery of CP violation involved many such interfaces, bringing together experimental and detection apparatuses, analysis techniques including Monte Carlo simulations, as well as cutting edge theoretical developments in quantum field theory and the standard model of physics. Negotiations occurred at all of these interfaces, ultimately leading to Fitch and Cronin's conclusion that T-symmetry is occasionally violated. How did the concept of time take shape among these interfaces? As described above, time entered into the experiment on multiple registers, and there was no cleanly circumscribed concept of time within this arrangement that could be easily offered to philosophical interrogation. Time was a messy concept,

[161] Galison, *How Experiments End*, 11.

and particle physics did not have obvious resources for describing it in such a way that it could become a clearly defined object of philosophical investigation.

In his 2005 book *Drawing Theories Apart*, Kaiser continues the conversation about the nature of theoretical entities in postwar particle physics.[162] Kaiser emphasizes the practical situations in which theoretical constructs have been put to use, demonstrating the various ways in which theories gain meaning through the specific sets tools, practices, and objectives belonging to subgroups of particle physics. Further, Kaiser shows how these tools have been transmitted through pedagogical means, such as textbooks and the training of postdocs. He claims that theoretical entities cannot be understood separately from the practices in which they are embedded and the pedagogical traditions through which they are transmitted. How did the concept of time gain meaning in terms of the specific practices of Fitch and Cronin's experiment and analysis? Further, could this meaning be transmitted to different discourse about physics, for which philosophical questions about time were germane? In practice, time in particle physics was at once a variable written down in equations, an observable entity emerging from statistical analyses like Monte Carlo simulations, and a relationship between boundary conditions. Due to the messy nature of the concept of time within particle physics, it could not easily be transported between contexts, nor was there a pedagogical tradition to facilitate its movement to discourses in which philosophical questions were more freely asked and answered. Without a mechanism for transportation of the concept of time, as presupposed in the experimental and theoretical practice of particle physics, into to a field

[162] David Kaiser, *Drawing Theories Apart* (Chicago: University of Chicago Press, 2005).

more amenable to philosophical speculation, the possibility for philosophical insight into time, deriving from CP violation, remained vague.

Time, as an object at stake within the context of the discovery of CP violation, existed at the interface of many elements, including experimental techniques, instruments, and theoretical principles. Further, time did not exist as a singular entity at this intersection, but was a messy composite conditioned by a variety of presuppositions. From the technical standpoint of the concept of time-reversal symmetry, time could not easily be transported into other discourses. It lacked the coherency, as well as the mechanism, to be transported into more philosophical discourses about time reversal in other fields of physics. Nevertheless, as in chapter one, the physicists involved in the discovery of CP violation presupposed that time was a consistent conceptual entity, commensurable between contexts, and capable of being understood in profound ways. This helps explain why the implications of CP violation for the concept of time never became a well-articulated line of research, but was nevertheless identified as one of the major sources of significance of Fitch and Cronin's result.

6. Conclusion

The experimental discovery of CP violation, and the way in which physicists such as Fitch and Cronin justified its significance, reveals several interesting aspects of the identity of particle physicists in the postwar period, as well as the concept of time as they understood and used it. The technical nature of the experiment, when combined with the justification of the result in terms of fundamental insights, shows that during this period the boundaries between a more speculative, "philosophical" understanding of physics and an experimental, technical, and pragmatic culture of physics, were shifting.

Fitch and Cronin both spoke of the discovery of CP violation as deeply profound for a variety reasons, high among them the fact that it impacted the concept time. However, they did not themselves delve into the profound questions raised, nor did they have a clear sense of what form these questions could take. They were still very much a part of the pragmatic mainstream of particle physics, which did not incorporate a form of philosophical engagement within the boundaries of physics. To be openly philosophical would have placed them outside of the pragmatic culture of particle physics within which they were firmly rooted. However, traces of a philosophical sensibility existed in the ways in which they discussed the stakes of their work. These physicists had a complicated sense of their professional identities with respect to philosophy and pragmatism. While highly pragmatic in practice, the concern with fundamentality that came with the field of particle physics created a place within their conceptions of physics for philosophical insight into the nature of basic concepts, at the level of justification.

Further, the framework of particle physics, and the ways in which multiple understandings of time came together at interfaces between technical, conceptual, mathematical, and experimental practices, made it difficult for thinkers to frame speculative questions about the nature of fundamental concepts. Without a means for interfacing the stakes of the experiment with other discourses surrounding time, physics, and symmetry, there was no context available for probing implications for the nature of time. Nevertheless, by going against accepted notions of the behavior fundamental laws under time reversal, considered among particle physicists in the early 1960s to be fundamental, CP violation placed the concept of time as a variable in the laws of particle physics at stake.

In chapter one I considered developments in physics that challenged the concept of time cast as a unit of measure. I drew attention to many of the contingencies that conditioned the concepts of a “clock” and a “second” in the postwar period, showing how instrumental advances in atomic clock technology implicitly changed physicists' understandings of these concepts. I showed time to be a heterogeneous concept within the context of atomic timekeeping; however, the chapter also noted that alongside these various and changing timekeeping concepts was a presupposition about the nature of time as a consistent and unproblematic physical variable. While timekeeping concepts were challenged in the episodes discussed in chapter one, this presupposition about time as a variable was not overtly at stake. In chapter two, I have considered an experiment, carried out only a few years after the development of the first atomic clocks, which explicitly challenged accepted notions about the time as a physical variable. Prior to the experiment, physical laws were thought to treat the two possible orientations of “t” symmetrically; the experiment showed this was not always the case. Thus, the theoretical variable “t” was itself at stake. Fitch, Cronin, and others identified the profound stakes of the experiment, including insight into the concept of time; however, they did not pursue any related lines of questioning. This can be explained by the particular identities of this group of physicists in relation to philosophical and pragmatic sensibilities; further it can be attributed the nature of these physicists' many presuppositions about time. Chapter two builds on chapter one to reveal a further dimension to the ways physicists conceptualized time during the postwar period, in this case within the complex framework of experimental and theoretical practices of particle physics.

The direction of time is arguably one of the most fundamental characteristics of time, and often described as the characteristic that differentiates time from space. In this chapter I have unpacked some of the presuppositions that condition the concept of time in relation to its directionality, showing how the nature of time's directionality was called into question in postwar American physics. Further, I show how, like in chapter one, physicists still upheld a presupposition about a truth of time, as well as a presupposition about time as a consistent concept. At an even more basic level, Fitch and Cronin presupposed that the time of theory exists, whatever the nature of its directionality with respect to physical laws. The directionality of time was called into question by the discovery of CP violation, however vaguely, but the existence of time as a meaningful concept was never at stake. Chapter three looks to a group of physicists, much more open to philosophical speculation, who explicitly questioned the presupposition that time exists in the first place. This will add further granularity to the landscape of professional identities of physicists in the postwar United States, as well as a further dimension to the heterogeneous concept of time as understood by physicists during this period.

CHAPTER THREE

Time and Quantum Gravity: Wheeler, DeWitt, and the "Equation of the Universe"

1. Introduction

For a few hours during an afternoon in the mid-1960s, Bryce DeWitt and John Wheeler met in the Raleigh Durham airport to discuss gravity. At the time DeWitt was director of the Institute of Field Physics at the University of North Carolina in Chapel Hill, a position Wheeler had helped him secure. Wheeler, a renowned Physics professor at Princeton University, had for some time been occupied with the question of how to unite gravity and quantum theory. Wheeler had arranged for the meeting, hoping to use a layover at Raleigh Durham to discuss his ideas about quantum gravity with DeWitt. While at the airport, DeWitt scribbled an equation on a piece of paper to facilitate the discussion. When Wheeler saw the equation he immediately became excited, believing it to be *the* equation of quantum gravity.[163] Wheeler presented the equation at several talks in the following years,[164] and DeWitt published it in the first of a trilogy of papers on

[163] See Interview of Bryce DeWitt by Kenneth W. Ford on 02/28/1995, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD USA, http://www.aip.org/history/ohilist/23199.html .

[164] See for example, John Wheeler, "Superspace and the Nature of Quantum Geometrodynamics," *Battelle Rencontres: 1967 Lectures in Mathematics and Physics*, Eds. Cecile Dewitt and John Wheeler (New York: Benjamin, 1968).

quantum gravity that appeared in *Physical Review* in 1967.[165] Due to the circumstances in which it was first written down, it came to be known as the “Wheeler-DeWitt” equation. However, it has also been referred to as the “Einstein-Schrodinger equation,” the “Equation of the Universe,” or as DeWitt later came to call it, “that damned equation.”[166]

In DeWitt’s 1967 paper, the equation takes the form:

$$\left(G_{ijkl}\frac{\delta}{\delta\gamma_{ij}}\frac{\delta}{\delta\gamma_{kl}}+\gamma^{1/2\,(3)}R\right)\Psi\left[{}^{(3)}g\right]=0\,.$$

At the most general level it is a quantum wave equation, describing the evolution of a quantum system in time, applied to the spacetime manifold given by Einstein’s gravitational field equations. The premise of the equation already suggests a potential difficulty concerning the concept of time in a quantum theory of gravity. That is, what does it mean for “spacetime” to evolve in “time”? In order to apply a quantum wave equation to spacetime, time needs to be singled out for special treatment as the background for the time-evolution of the system. The Wheeler-DeWitt equation gets around this difficulty by describing the time-variation of a 3-dimensional cross-section of 4-dimensional spacetime, as indicated by the terms ${}^{(3)}R$ and ${}^{(3)}g$; further, the Latin indices *ijkl* convey that this 3-dimensional cross-section is “space-like”. In this equation time and space have been separated from one another as distinct elements of spacetime, with time

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[165] Bryce S. DeWitt, “Quantum Theory of Gravity. I. The Canonical Theory,” *Physical Review* **160** (1967): 1113-1148

[166] See Bryce DeWitt, “The Quantum and Gravity: The Wheeler-DeWitt Equation”, *Proceedings of the Eighth Marcel Grossmann Conference, The Hebrew University, Jerusalem* (Singapore: World Scientific, 1997).

serving as the background for variation within the system; however, such a separation is problematic for the traditional concept of spacetime in relativity.[167] This issue occupied Wheeler, DeWitt, and the community of physicists working on quantum gravity in the 1950s and 60s, leading them to grapple with questions such as: what is the relationship between time and space in general relativity? What is "time-evolution" in quantum mechanics? Does the concept of time have any meaning in the context of the universe as a whole? Can efforts to unify quantum theory and general relativity provide new insights into the nature of time in physics?

Explicitly ontological questions about the nature of time were central to Wheeler and DeWitt's work on quantum gravity in the late 1960s. Such questions were seamlessly integrated into the 1967 paper in which DeWitt first published the equation, appearing directly alongside highly technical mathematical derivations. For example, in a section of the paper entitled "A Wave Packet for the Universe; The Concept of Time," DeWitt derives an equation for a "wave packet for the universe". Immediately following his technical derivation he draws the conclusion that "'time' is only a phenomenological concept, useful under certain circumstances."[168] He goes on to discuss the meaning of

[167] The idea that space and time cannot be treated as having independent realities, but rather belong to a unified 4-dimensional "spacetime," was central to Hermann Minkowski's 1908 interpretation of Einstein's special theory of relativity. Further, Einstein's 1916 publication of the theory of general relativity placed 4-dimensional curved spacetime at the center of the physical understanding of gravity. See Hermann Minkowski, "Space and Time," in *The Principle of Relativity*, ed. H. Lorentz et. al, (Mineola, NY: Dover Publications Inc., 1952), p. 75; Albert Einstein, "The Foundation of the General Theory of Relativity," in *The Principle of Relativity*, ed. H. Lorentz et. al, (Mineola, NY: Dover Publications Inc., 1952), p. 109.

[168] DeWitt, "Quantum Gravity. I. The Canonical Theory," 1137.

this claim about time, moving between mathematical analyses and speculative interpretation.[169] Similarly, in a 1967 talk in which Wheeler makes one of his first recorded mentions of the equation (he refers to it as the Einstein-Schrodinger equation), Wheeler makes overtly interpretive, ontological claims about time. For example, after a discussion of how to define the background in a theory of quantum gravity, he claims:

> [O]ne has to forgo that view of nature in which every event, past, present, or future, occupies its preordained position in a grand catalog called "spacetime." There is no spacetime, there is no time, there is no before, there is no after. The question what happens "next" is without meaning.[170]

Wheeler and DeWitt's interpretive work in the 1960s led both to the conclusion that time does not have a precisely defined "reality" in a universe described by the Wheeler-DeWitt equation. While their interpretive pathways differed, and Wheeler's ontological claims about time were stronger than DeWitt's, both came to similar conclusions about time. In their work leading to the Wheeler-DeWitt equation, both opted to treat space and time as distinct in their formulation of Einstein's field equations. In doing so, they defined what they meant by "time". DeWitt defined time in relation to the contents of the universe, and Wheeler in terms of the relationship between 3-dimensional space and a 4-dimensional spacetime. When each physicist applied quantum principles to their definitions of time, they interpreted the result as implying that time was an approximate and secondary concept. DeWitt's calculation resulted in an apparently

[169] *Ibid.*, 1134.

[170] Wheeler, "Superspace and the Nature of Quantum Geometrodynamics", 253.

motionless universe described by the Wheeler-DeWitt equation, leading him to the conclusion that time is a "*phenomenological concept*". Wheeler found that when taken in the context of a quantum mechanical universe, the relation between 3-dimensional space and 4-dimensional spacetime became poorly defined, rendering the concept of spacetime itself meaningless. This led him to the conclusion that "*there is no time*".

Speculative and interpretive engagement with fundamental concepts such as time, particularly in relation to quantum gravity, occupied Wheeler and DeWitt well beyond the 1960s. For example, in a 1988 paper published by Wheeler in the *IBM Journal of Research and Development,* Wheeler makes the argument that time is an "illusion," largely based on insights drawn from his quantum gravity work from decades earlier.[171] Further, Wheeler and DeWitt were both major players in debates surrounding the Everett interpretation of quantum mechanics, which was put forward in Hugh Everett's 1957 dissertation, completed under Wheeler's supervision, and championed by DeWitt largely due to connections DeWitt saw between the Everett interpretation and quantum gravity.[172] This type of interpretive engagement with fundamental concepts has also been a feature of quantum gravity research programs that revisited the Wheeler-DeWitt equation in the decades following its conception, such as efforts in loop quantum

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[171] John Archibald Wheeler, "World as System Self-Sythesized by Quantum Networking," *IBM Journal of Research and Development,* **32** (1987): 4-15.

[172] Hugh Everett III, *On the Foundations of Quantum* Mechanics, Ph.D. thesis, Princeton University ; Bryce DeWitt, "Quantum Mechanics and Reality," *Physics Today*, **23(9)** (1970): 155–165.

gravity.[173] This chapter will trace this line of interpretive and speculative thinking, primarily concerning the nature of time in relation to the Wheeler-DeWitt equation, from the original publications and talks in which Wheeler and DeWitt derived and interpreted the equation, through Wheeler and DeWitt's later work, to its legacy in contemporary quantum gravity research.

Interpretive thinking about basic concepts such as time was not unusual among the small subgroup of physicists working on quantum gravity in the 1960s. However, this was not the case for all subgroups of physicists during this period, for example the experimental physicists, engineers, and astronomers working on atomic clocks discussed in chapter one of this dissertation and the experimental particle physicists working on time asymmetry discussed in chapter two. These other physicists were also working on topics directly related to the concept of time, and the meaning of "time" was implicitly at stake in their work; however, they did not explicitly question the nature of time *per se*. Wheeler and DeWitt identified themselves as theoretical as opposed to experimental physicists, which is one feature that distinguishes them from the subgroups discussed in the preceding chapters; however, even among theoretical physicists Wheeler and DeWitt's attitudes toward speculative thinking were far from typical. As physicists, they understood their roles as involving deep engagement with the nature of fundamental concepts, straddling the boundary between physics and philosophy. In autobiographical reminiscences composed later in life, Wheeler wrote:

[173] See Lee Smolin, *Three Roads to Quantum Gravity* (New York: Basic Books, 2001).

> I have not been able to stop puzzling over the riddle of existence. From the calculations and experiments that we call the nitty-gritty of our science to the most encompassing questions of philosophy, there is one unbroken chain of connection. There is no definable point along this chain where the truly curious physicist can say, "I go only this far and no farther.[174]

How did Wheeler and DeWitt understand their professional identities, as postwar American physicists, in relation to a perceived "chain of connection" between physics and "philosophy"? How did these professional identities relate to the questions they pursued in their work on quantum gravity, in particular surrounding the concept of time? How did they stand in relation to the professional identities of other subgroups of physicists from the same period?

Several historians of science have commented on Wheeler's tendency toward philosophically inflected physics, which was not typical among mainstream American physicists during the postwar period.[175] These scholars have largely attributed this tendency to Wheeler's relationship with Niels Bohr, which developed during a formative post-doctoral year Wheeler spent with Bohr in Copenhagen from 1934-35.[176] Further,

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[174] John Wheeler with Kenneth Ford, *Geons, Black Holes, and Quantum Foam: A Life in Physics* (New York, W.W. Norton & Company, 1998), 263.

[175] Silvan S. Schweber, "The Empiricist Temper Regnant: Theoretical Physics in the United States, 1920—1950," *Historical Studies in the Physical and Biological Sciences*, Vol. 17 (1986): 55-98; David Kaiser, *How the Hippies Saved Physics: Science, Counterculture, and the Quantum Revival* (New York: W.W. Norton and Company, 2011), and Peter Galison, "Structure of Crystal, Bucket of Dust," *Circles Disturbed, the Interplay of Mathematics and Narrative*, eds. Apostolos K. Doxiadēs and Barry Mazur (Princeton: Princeton University Press, 2012), 52-78.

[176] See for example, Schweber, "The Empiricist Temper Regnant," 95; Kaiser, *How the Hippies Saved Physics*, 75; Galison, "Structure of Crystal," 62.

historians have attributed the more typically “American” facets of Wheeler’s work – for example his interest in engineering, machines, and practical applications of theory –to Wheeler’s American upbringing and training, as well as his work on the Manhattan project and the Hydrogen bomb.[177] Wheeler’s experiences, training, and mentors go a long way toward explaining his identity as an American physicist who was, “quintessentially, the scientist who insistently cycled philosophical questions of meaning throughout the technical work.”[178] As for DeWitt, he chose to work on quantum gravity in the late 1940s, as the subject of his PhD research under the supervision of Julian Schwinger, and devoted his entire career to the topic. As a researcher interested in general relativity, DeWitt experienced hostility from the physics community early in his career.[179] DeWitt had difficulty securing a position within a physics department in the United States following his graduate work, and found himself as somewhat of an outsider. It was only with the help of Wheeler that DeWitt eventually obtained a faculty position at UNC Chapel Hill and established himself within the mainstream of physics. Wheeler’s sponsorship of DeWitt’s career, as well as his general promotion of general relativity research in the postwar period, helped carve a path for DeWitt as a quantum gravity researcher interested in questions about the meaning of fundamental concepts. Wheeler helped define the questions DeWitt asked about quantum gravity, as well as

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[177] Galison, “Structure of Crystal.”

[178] Galison, “Structure of Crystal,” 58.

[179] For DeWitt’s account of the hostility of the physics community toward general relativity, see AIP interview of DeWitt by Ford.

created a space within the physics community for Dewitt's interest in questioning the meaning of fundamental concepts.

Wheeler became interested in general relativity, quantum gravity, and the attendant ontological questions about time in the early 1950s, after having already earned a strong reputation within the American physics community. He had a secure position at Princeton, a philosophical sensibility nurtured by his relationship with Bohr, and was well situated to be bold in his lines of questioning. His set of experiences allowed him to carve out an identity for himself as a serious American physicist interested in fundamental questions about the meaning of time. Further, he helped create a space for other physicists, like DeWitt, to share in aspects this identity and flourish within the mainstream American physics community. One of the primary objectives of this chapter will be to unpack this vision of what it meant to be a philosophically engaged postwar American physicists, as exemplified by Wheeler and DeWitt's engagement with the concept of time.

This objective is part of the broader line of questioning, central to this dissertation, which asks how postwar American physicists carved out their professional identities in relation to interpretive and speculative engagement with fundamental concepts in physics, particularly the concept of time. During the postwar period subgroups of physicists assumed a variety of attitudes toward fundamental concepts. This was partly due to the different ways each subgroup took up particular aspects of the physics legacies they inherited, as well as the social and political contexts in which each group moved. The result was a multiplicity of understandings of what it meant to be a physicist, each corresponding to a different way of questioning and thinking about the nature of fundamental concepts such as time. By tracing questions about time surrounding the Wheeler-DeWitt equation, this chapter will shed light on one piece of the history of physicists' varied, complicated, and changing relationship to their fundamental concepts and their discipline.

The central question of how and why physicists questioned the nature of fundamental concepts, and what this reveals about their professional identities, builds on many interesting works in the history of science by thinkers such as Sam Schweber, Alexi Assimus, Nancy Cartwright, Peter Galison, and David Kaiser.[180] As discussed in the introduction to this dissertation, these scholars give various historical explanations for the existence of a tension in American physics between pragmatism and a more expansive approach to physics and its fundamental concepts, as well as track various incarnations of this tension over the course of the twentieth century. For example, David Kaiser argued in his 2011 book *How the Hippies Saved Physics: Science, Counterculture, and the Quantum Revival,* that the speculative, interpretive impulse was largely absent from mainstream American physics during the postwar period, but nevertheless stayed alive in fringe and countercultural moments, to eventually reenter the mainstream. This project builds on Kaiser's work by focusing on mainstream physicists during the same

[180] Schweber, "The Empiricist Temper Regnant"; Alexi Assmus, "The Americanization of Molecular Physics," *Historical Studies in the Physical and Biological Sciences*, Vol. 23, No. 1 (1992): 1 – 34; Nancy Cartwright, "Philosophical Problems of Quantum Theory", in *The Probabilistic Revolution, V2*, ed. Lorenz Kruger (Cambridge, MA: MIT Press, 1990); Peter Galison, *How Experiments End* (Chicago: University of Chicago Press, 1987); Peter Galison, *Image and Logic* (Chicago: University of Chicago Press, 1997); Kaiser, *How the Hippies Saved Physics*.

period, and how they grappled with the tension between engaging deep fundamental questions and a form of postwar pragmatism.

The subjects of this chapter - Wheeler, DeWitt, the equation that bears their name and its legacy – provide an excellent point of entry to the set of questions that motivate this project. Wheeler and DeWitt's explicit engagement in interpretive questioning about the concept of time stands in contrast to other more pragmatic attitudes of physicists during this period. With an eye to understanding and unpacking the interpretive and speculative features of their work and its legacy, this chapter will take a close look at DeWitt and Wheeler's thinking about time in relation to the Wheeler-DeWitt equation and quantum gravity, as continuation of this line of thinking among later physicists. Section two will provide a brief outline of the quantum gravity research program up until the point of DeWitt's publication of the Wheeler-DeWitt equation in 1967, as well as a short biographical sketch of Wheeler, DeWitt, and their professional relationship. Section three will look to DeWitt's approach to time and quantum gravity, including a close look at the first installment of the 1967 trilogy and DeWitt's engagement with the Everett interpretation of quantum mechanics. Section four will look to Wheeler, the 1967 lecture in which he introduced the Wheeler-DeWitt equation, and a sample of his later writings dealing with time and quantum gravity. The fifth section will consider how Wheeler and DeWitt's thinking about time was taken up in later quantum gravity research programs, looking particularly to Lee Smolin and Julian Barbour's work on loop quantum gravity. After analyzing how Wheeler, DeWitt, and those who followed in their footsteps asked questions about the nature of fundamental concepts such as time, I will look to how this questioning informed and was informed by these physicists' conceptions of what it meant to be a physicist. Set in relation to how other groups were asking and answering questions about time, a heterogeneous picture of the identity of physicists in the post war United States, as well as the multifaceted nature of the concept of time in physics, will emerge.

2. Background

2.1 Quantum gravity research 1930-1967

Prior to 1967, most attempts to develop a quantum theory of gravity fell into one of two camps, each corresponding to a particular approach to defining the background for gravitational dynamics. The first, termed the "canonical approach," singled out time for special treatment, considering how a space-like cross-section of spacetime evolves in time. The second, termed the "covariant approach," designated a particular spacetime background – often a flat Minkowskian spacetime – against which to represent the dynamics of quantum gravity. DeWitt and Wheeler contributed to both approaches in the 1950s and 60s, the Wheeler-DeWitt equation belonging to the canonical tradition. The first paper in DeWitt's 1967 trilogy, in which the Wheeler-DeWitt equation appears, is devoted to the canonical approach, while the second and third papers of the trilogy focus on the covariant approach and its applications. At the time of the publication of the trilogy DeWitt did not see any formal connection between the two approaches. As he wrote in the introduction to the first paper, "the so-called manifestly covariant theory […] differs utterly in its structure from the canonical theory, and so far no one has established a rigorous mathematical link between the two."[181] This being said, he noted that among some "it is believed that the two theories are merely two versions of the same theory, expressed in different language, but no one knows for sure."[182]

The canonical and covariant approaches to quantum gravity have been characterized in variety of ways. For example, in the 1967 trilogy DeWitt characterized the canonical theory as "describing the quantum behavior of 3-space regarded as a time-varying geometrical object, and the covariant [as] describing the behavior of real and virtual gravitons propagating in this object."[183] Further, he differentiated the two approaches according to the types of questions they asked and insights they produced, claiming that "the canonical theory leads to conclusions about "amplitudes of different 3-geometrics or 'the wave function of the universe'. The covariant theory, on the other hand, concerns itself with 'micro-processes' such as scattering, vacuum polarization, etc."[184] In 2001, Lee Smolin described the two traditions in his popular book *Three Roads to Quantum Gravity*, characterizing the canonical approach as starting with "the essential principles of Einstein's theory of General Relativity and seek[ing] to modify them to include quantum phenomena," and the covariant approach as starting from "quantum theory, in which most of the ideas and methods were developed first in other parts of

[181] DeWitt, "Quantum Theory of Gravity I: The Canonical Theory," 1115.

[182] DeWitt, "Quantum Theory of Gravity II: The Manifestly Covariant Theory," 1197.

[183] DeWitt, "Quantum Theory of Gravity I: The Canonical Theory," 1115.

[184] *Ibid*.,1239.

quantum theory."[185] The two approaches thus differ in their points of departure and the questions they ask, in addition to the choice of background. The choice of background, however, is the most important distinction for the purposes of this chapter, due to its direct relevance to the interpretation of time and spacetime. The fact that the canonical approach singled out time for special treatment was deeply troubling to DeWitt, who wrote in 1967 that this aspect of the canonical approach "run[s] counter to the spirit of any relativistic theory."[186]

The first canonical efforts to develop a quantum theory of gravity were undertaken by Leon Rosenfeld, who applied quantum mechanical equations to a linearized form of Einstein's relativistic field equations in the 1930s.[187] Rosenfeld encountered many difficulties while undertaking this effort, and defined many of the problems with which future physicists working on quantum gravity would grapple. In particular, Rosenfeld articulated the problems associated with defining "observables" in a quantum theory of gravity, coming to the conclusion that such observables would only be present at extremely high energies.[188,] After Rosenfeld's pioneering work, not many physicists worked on canonical quantum gravity until the 1950s, when work by physicist Peter Bergmann launched a new period of activity in the field. Bergmann, along with a

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[185] Smolin, *Three roads to Quantum* Gravity, 9.

[186] DeWitt, "Quantum Gravity. I. The Canonical Theory," 1114.

[187] See and Carlo Rovelli, "Notes for a Brief History of Quantum Gravity," presented at the *9th Marcel Grossmann Meeting,* July 2000 (revised 2008), 7; and DeWitt, "Quantum Gravity I: The Canonical Thoery, 1113;

[188] L Rosenfeld, Ann. Physik **5,** 133 (1930); Z. Physik **65***,* 589 (1930).

group of colleagues and graduate students, began a concerted program to define the observables of quantum gravity, as well as to quantize the non-linearized equations of general relativity.[189]

Meanwhile, advances in non-quantized general relativity and its classical canonical structure, particularly a Hamiltonian formulation of the equations of general relativity by Arnowitz, Deser, and Misner (ADM),[190] showed promise to move the field of gravitation physics forward. It was based on work in non-quantized general relativity, and particularly a reformulation of ADM written down by Asher Peres,[191] that served as the basis for the equation DeWitt wrote down for Wheeler at the Raleigh-Durham airport in the 1960s.

As for the covariant approach, developments also began in the 1930s, with the work of physicists such as Rosenfeld, Bronstein, Fierz and Pauli.[192] Major advances were made in covariant quantum gravity in the 1960s, particularly with the work of DeWitt and Feynman on the Feynman rules for general relativity.[193] Although the history

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[189] *Ibid*.

[190] Arnowitt, R.; Deser, S.; Misner, C., "Dynamical Structure and Definition of Energy in General Relativity," *Physical Review* **116** (1959): 1322–1330.

[191] Asher Peres, *Nuovo Cimento* **26** (1962): 53.

[192] See Rovelli, "Notes on a Brief History of Quantum Gravity," 8; and John Stachel, "Early History of Quantum Gravity" in *Black Holes, Gravitational Radiation and the Universe*, Eds. Bala R. Iyer and Biplap Bhawal (Dordrecht: Kluwer Academic Publishers, 1999).

[193] See for example Bryce DeWitt, "Quantum Theory of Gravity II: The Manifestly Covariant Theory," Phys Rev, 160 (1967): 119.

of the covariant approach is a rich one, the canonical approach, and the way time has been conceptualized within it, will be the focus of this chapter.

2.2 Wheeler, DeWitt, and Gravity

John Wheeler was born in Jacksonville Florida in 1911. He became interested in physics and engineering early in life, and enrolled as an engineering student at Johns Hopkins University at the age of 16.[194] After his first semester Wheeler transferred his major to physics, which he found more inspiring than engineering.[195] He was "enchanted" with physics and pursued a PhD in atomic physics at Johns Hopkins under the supervision of Karl Herzfeld.[196] Following his graduate work, Wheeler completed a postdoctoral year at New York University (NYU), under the supervision of Gregory Breit; following his time at NYU, Wheeler spent a second postdoctoral year in Copenhagen under the supervision Niels Bohr. Breit and Bohr had markedly different approaches styles of doing physics, and each left their mark on Wheeler. Wheeler reflected in his autobiography:

> Working with Bohr and working with Breit were complementary experiences. […] Breit taught me new mathematical and calculational techniques. Bohr taught me a new way of looking at the world, a new way of raising questions. […] I have always loved pushing the mathematics beyond formalism, to get numerical results that can be turned into pictures and compared with experiments. At the same time, I have had a lifelong fascination with the meaning of the quantum and

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[194] See AIP interview of Wheeler by Ford.

[195] *Ibid.*

[196] *Ibid.*

> the urge always to think about what physics might be like twenty years hence, not just the day after tomorrow.[197]

After completing his year in Copenhagen, Wheeler joined the physics department at UNC Chapel Hill in 1935 as an assistant professor, where he remained until 1938 when he took a position at Princeton. During World War II Wheeler worked closely with engineers and physicists at the Metallurgical Laboratory in Chicago, as well as at the Hanford reactor site in Washington. Wheeler returned to Princeton after the war, but left again to spend a year at Los Alamos working on the Hydrogen bomb from 1950-1951. After leaving Los Alamos, he continued his H-bomb work at Princeton for several years.[198]

During the early 1950s, as his work on the hydrogen bomb was winding down, Wheeler began to take an interest in general relativity. He had little experience with general relativity at the time, but it was a field which had long fascinated him.[199] Wheeler decided to teach a class on the subject in 1952, as well as compose a textbook on the material.[200] As he explored relativity, he became particularly interested in possible relationships between quantum theory and general relativity. As he later reminisced, "I had been so enthusiastic for the sum-over-histories way of describing quantum mechanics and the transition from quantum mechanics to classical mechanics that I couldn't help

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[197] Wheeler, *Geons,* 139.

[198] See AIP interview of Wheeler by Ford.

[199] *Ibid.*

[200] *Ibid.*

looking for a similar transition in the case of relativity."[201] Upon taking interest in the subject, Wheeler became aware of DeWitt's work on quantum gravity, in particular DeWitt's 1949 doctoral thesis on the subject. Wheeler went to visit DeWitt to discuss the thesis and the topic in general, which began their personal and professional friendship.[202]

DeWitt was born in Dinuba California in 1923. His family moved to Massachusetts when he was 12, and he enrolled at Harvard at the age of 16. DeWitt graduated from Harvard with a degree in physics in 1943, after which he briefly worked on the Manhattan project at the Calutron at Berkeley. However, after seven months at the Calutron he asked to be released from the Manhattan project and enlisted in the navy. Following the war he returned to Harvard to complete his Ph.D. under the supervision of Julian Schwinger. He chose the quantization of the gravitational field as his topic, which defined his research for the remainder of his career.[203] Quantum gravity was an uncommon choice of topic for a young physicist. DeWitt later reflected that "at the beginning my goal was regarded by colleagues as [] indecent: not what 'real' physicists do."[204]

Upon completion of his Ph.D., DeWitt spent time as a postdoctoral fellow at the Institute of Advanced Study at Princeton, the ETH in Zurich, and the Tata Institute of

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[201] *Ibid.*

[202] *Ibid.*

[203] See "A biographical Memoir by Steven Weinberg," in *The Pursuit of Quantum Gravity Memoirs of Bryce DeWitt from 1946 to 2004*, Ed. Cecile DeWitt-Morette (New York: Springer, 2011).

[204] Bryce DeWitt, "Why Physics?" in *The Pursuit of Quantum Gravity*, 4.

Fundamental Research in Bombay. DeWitt's time in Bombay was interrupted by illness, but he eventually completed his final post-doc in 1952. Upon returning to the United States, DeWitt struggled to find a job, partially due to the unpopularity of general relativity research at the time, as well as his time spent abroad. As he recalls, it was difficult for him to begin his career as an American physicist, considering "how hostile the physics community was, in [the 1950s], to persons who studied general relativity."[205] Unable to find an academic appointment, DeWitt took a job at the nuclear weapons laboratory in Livermore in 1952, while his wife Cecile DeWitt-Morette, a physicist and mathematician, worked in Europe.[206]

In the meantime, Wheeler had taken a notice in DeWitt's work. Wheeler became a sponsor for DeWitt and was instrumental in securing positions for both DeWitts at the University of North Carolina (UNC) in Chapel Hill.[207] Bryce came on as a visiting research professor and director of the Institute of Field Physics at UNC, and was later promoted to full professor. Cecile also began as assistant visiting professor but was later demoted to lecturer.[208] Largely due to UNC's unwillingness to give Cecile a full position,

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205 Bryce DeWitt, "Why Quantum Gravity? Why Link Quantum Gravity and Bryce DeWitt's Memoirs?" in *The Pursuit of Quantum Gravity*, 6.

206 See interview of DeWitts by Ford, AIP.

207 John Wheeler to Harris Purks (acting president of University of North Carolina, Chapel Hill), 25 November 1955. Wheeler Papers, American Philosophical Society.

208 Cecille DeWitt-Morette, *The Pursuit of Quantum Gravity* (New York, Springer, 2011), 127.

the DeWitts began looking for other opportunities in the 1970s.[209] They decided to move University of Texas (UT) Austin in 1972, which had a vibrant group working on relativity and a tenured position for Cecile. Bryce was eventually instrumental in bringing Wheeler to UT Austin in 1976, after Wheeler retired from Princeton.[210]

The relationship between Wheeler and DeWitt had a great influence on DeWitt's thinking about quantum gravity, particularly his work on the canonical approach. He later reminisced that all of his thinking about the canonical theory of quantum gravity, and particularly the first part of the 1967 trilogy, was a direct result of his conversations with Wheeler.[211]

3. DeWitt

3.1 The 1967 Trilogy: the road to publication

DeWitt's 1967 trilogy was intended as a review of the state of progress in quantum gravity at the time, as well as an application of current theory to specific scenarios.[212] It

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[209] *Ibid.* Also referenced in John Wheeler to James H. Crawford (chairman, Physics department UNC Chapel Hill), 8 February 1971. Wheeler Papers, American Philosophical Society.

[210] Bryce DeWitt to John Wheeler, 16 April 1976. Wheeler papers, American Philosophical Society.

[211] Bryce DeWitt, "The Quantum and Gravity: The Wheeler-DeWitt Equation," *The Eighth Marcel Grossman Meeting on Recent Developments in Theoretical and Experimental General Relativity, Gravitation, and Relativistic Field Theories*, (Singapore: World Scientific, 1997), 6.

[212] Bryce DeWitt to John Wheeler, 17 June 1966. Wheeler papers, American Philosophical Society.

was a project to which DeWitt devoted a great deal of time and effort, and in which he was personally invested. In a letter to Wheeler in dated June 17, 1966, DeWitt confessed, “The article has been a year and a half in preparation and this has meant long nights spent in my office rather than at home, and a neglected wife and children can testify that this project was not undertaken lightly.”[213] It was thus disheartening to DeWitt when he encountered some resistance to the publication of the piece from editors and reviewers, including Wheeler himself, who reviewed the publication when it was originally submitted. Nevertheless, the piece was eventually published and heralded as a masterpiece. In 1975 Wheeler described the three papers as “Bible references in the field.”[214]

The publication was not initially intended as a trilogy for *Physical Review*, but was first submitted as a single long paper to the *Review of Modern Physics*. However, the editor of *Review of Modern Physics* at the time, E.U. Condon, had major concerns with DeWitt’s submission in its original form. Condon contacted Wheeler, requesting that he review the paper and evaluate whether Condon’s concerns were justified. In a letter to Wheeler dated May 17, 1966, Condon wrote of DeWitt’s submission: “I find it extremely difficult to read and, although I am getting very, very old, admittedly, I doubt if it is intelligible to anyone except real specialists in this game like yourself.”[215] Further,

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[213] *Ibid.*

[214] John Wheeler to Gordon Ray, 24 January, 1975. Wheeler papers, American Philosophical Society.

[215] Ed Condon to John Wheeler, 17 May 1966. Wheeler papers, American Institute of Physics.

Condon was concerned that the paper was not a literature review but constituted original research, based primarily on the grounds that a title-page footnote stated "this research was supported by […]".[216] Further, Condon was concerned that the Institute of Field Physics at UNC Chapel Hill had already published it.[217] Despite these concerns, Condon wrote to Wheeler: "there are always borderline cases so I would not be rigid (despite my own inability to understand it!) if it would be of such great value to physics that we should publish it." Further, he added: "I like the title and wish I could publish a paper on the subject that could really be read by the laity."[218]

Wheeler was sympathetic to Condon's concerns about DeWitt's submission, and contacted DeWitt by phone to discuss.[219] In response to the phone conversation, DeWitt wrote Wheeler a long letter in defense of his publication. In the letter DeWitt went through each section of his article, pointing out which portions constituted "review" and which "original contribution"; further, he did the same for several recent *Review of Modern Physics* articles, showing the ratio of review to original contribution in his submission to be similar to that of other articles the journal had published.[220] After presenting his case on this point, DeWitt made it clear that he was disappointed Wheeler

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[216] *Ibid.*

[217] *Ibid.*

[218] *Ibid.*

[219] Bryce DeWitt to John Wheeler, 17 June 1966. Wheeler papers, American Philosophical Society.

[220] *Ibid*.

wasn't more receptive to the publication, and upset at the suggestion that it was too technical. He wrote passionately to Wheeler,

> Should I really take my work, chop it into pieces, jazz it up with speculative gimmicks, and feed it to the public in a steady stream of advertising copy? […] I would be very happy if you would take an item from my article --- any item – and pick it to pieces, showing why you think it is wrong and in what way it might be corrected. Then I could write a better article. But to have you reject the article on grounds which I really have not understood, while implying that pressure of time prevents you from taking more than a cursory look at it, is unfair. Crackpots are treated in this fashion. It will indeed be nice to have a chance to go over parts of it with you next fall. But that will be next fall, and the article was submitted in May.[221]

Wheeler eventually suggested that, with a bit of reworking, the article would be suitable for publication in *Physical Review* as opposed to *Review of Modern Physics.* In particular, he recommended the paper be divided into three installments, arguing that the canonical and covariant theories were so different that their inclusion in a single publication compromised its coherence. Wheeler felt this division would make the paper accessible to readers and suitable for publication.[222] DeWitt followed this advice, and the trilogy was accepted for publication in *Physical Review* in three installments in 1967.[223]

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[221] *Ibid.*

[222] John Wheeler to Bryce DeWitt, 11 July 1966. Wheeler papers, American Philosophical Society.

[223] Bryce DeWitt to Simon Pasternack (Editor, Physical Review) 20 July 1966. Wheeler paper, American Philosophical Society.

3.2 Quantum Theory of Gravity I: The Canonical theory

The first installment of the trilogy is devoted to canonical quantum gravity, and contains the equation that has come to be referred to as the Wheeler-DeWitt equation. It should be noted that DeWitt believed the canonical approach to be a less promising route to a theory of quantum gravity than the covariant approach. In 1967 DeWitt claimed that, because the canonical approach singled out time for special treatment, it was not in keeping with the spirit of general relativity.[224] Further, he wrote later in life that he had primarily been interested in the canonical approach in the 1960s because of "the bizarre logical pathways one has to follow in interpreting it," rather than due to a belief that it was a viable path forward in developing a quantum theory of gravity.[225] However, DeWitt's interpretive pathway is of great interest for the purposes of this chapter. In this section I will analyze the canonical paper, paying close attention to the elements DeWitt described as the "bizarre features of the formalism […] which are of possible cosmological and even metaphysical significance."[226]

The first paper of the trilogy, which DeWitt describes in the introduction as "the direct outcome of conversations with Wheeler," is organized into ten sections. The first section constitutes a historical introduction to quantum gravity, focusing on the canonical tradition, with emphasis on the work of Rosenfeld in the 1930s and Bergmann in the

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[224] DeWitt, "Quantum Theory of Gravity I: The Canonical Theory," 1114.

[225] DeWitt, "The Quantum and Gravity," 7.

[226] DeWitt "Quantum Theory of Gravity II: The Manifestly Covariant Theory," 1195.

1950s.[227] Sections two and three develop Einstein's gravitational field equations in the classical canonical representation, building on the work of physicists including Arnowitz, Deser, Misner, Peres, and Wheeler.[228] This involves an equation to describe the "curvature of the hypersurface as viewed from the 4th dimensional space-time in which it is embedded," which is the space-like cross-section central to the canonical approach.[229] DeWitt introduces "quantum constraints" in section four, immediately noting that these constraints are "often a source of puzzlement and confusion."[230] The issue he is referring to lies with the fact that when the constraints are applied, the equations appear to imply that there is no time-evolution in the universe. This could lead to the "conclusion that nothing ever happens in quantum gravidynamics, that the quantum theory can never yield anything but a static picture of the world."[231] This possibility – that the quantum universe is static and timeless – is unattractive to DeWitt, who makes efforts throughout the remainder of the paper to show that there is a role for time in a quantum universe, even if this involves a new interpretation of what time "is".

DeWitt first addresses the issue by suggesting the seeming timelessness of the quantum universe is an artifact of the arbitrary choice of coordinates, particularly and the space-like cross-section x^0=constant. He writes:

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[227] DeWitt, "Quantum Theory of Gravity I: The Canonical Theory," 1113-1114.

[228] *Ibid.*1116.

[229] *Ibid.*, 1117.

[230] *Ibid.,* 1119.

[231] *Ibid.*

> instead of regarding this […] as implying that the universe is static we shall interpret it as informing us that the coordinate labels x^μ are really irrelevant. Physical significance can be ascribed only to the intrinsic dynamics of the world, and for the description of this we need some kind of intrinsic coordinization based either on the geometry or the contents of the universe.[232]

In other words, the seemingly static nature of the universe is due the arbitrary designation of space-like and time-like coordinates, which assumes that time is external to the system. DeWitt goes on in section five to develop the quantized gravitational field equations, leading to the “functional wave equation” of the universe that has come to be referred to as the Wheeler-DeWitt equation. Returning to the issue of the apparently static universe, he suggests the possibility of reintroducing a time-like coordinate. This would support the idea that an “intrinsic time” exists, and that the universe “does have dynamic content.”[233]

In section six, DeWitt discusses the phenomenon of “gravitational collapse.”[234] In sections seven, eight, and nine, he applies the Wheeler-DeWitt equation to the Friedmann universe. DeWitt chooses the Friedmann model for these final sections because it is “the simplest classical model which exhibits the collapse phenomenon.”[235] Further, the Friedmann universe allows DeWitt to find solutions to the Wheeler-DeWitt equation, using approximation techniques. Although greatly simplified, the approximate equations

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[232] *Ibid.,* 1120.

[233] *Ibid.*, 1124.

[234] Wheeler would later coin the term “black hole” to describe gravitational collapse.

[235] DeWitt, “Quantum Theory of Gravity I: The Canonical Theory,” 1130.

describing the Friedmann universe provide a point of entry for DeWitt to discuss some features of a universe described by a "universal wave function," including the nature of time.

In section seven, after solving the Wheeler-DeWitt equation for the Friedman universe using approximate techniques, DeWitt introduces the idea that within the universe "the collective internal motion permits the particle ensemble to be used as a clock."[236] This idea is developed further in section eight, in which DeWitt considers three possible wave-packets to describe such a universe. He describes these wave-packets as providing "three distinct mathematical windows from which to view the Friedmann world."[237] He goes on:

> From one window the material content of the universe is seen as a clock for determining the dynamical behavior of the world geometry. From another it is the geometry which appears as a clock for determining the dynamical behavior of the material content. From the third the geometry and the material content appear on equal footing, each one correlated in a certain manner with the other.

DeWitt concludes, "The third window is to be preferred as most accurately revealing the physics of the quantized Friedmann model."[238] This is firstly because the time variables in the first two windows "are not rigorously observable and hence cannot yield a measure of proper time which is valid under all circumstances."[239] However, DeWitt notes that

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[236] *Ibid.,* 1132.

[237] *Ibid.*, 1136.

[238] *Ibid.*

[239] *Ibid.*

even in the third window the concept of time is problematic. This is due to “the fact that the wave packets [in the first two windows] spread in ‘time,’ whereas the packet [in the third window] does not.”[240] This allows DeWitt to draw the final conclusion that, due to the absence of spreading, “we may say that ‘time’ is only a phenomenological concept, useful under certain circumstances.”[241]

DeWitt continues: “it is not necessary to drag in the whole universe to argue for the phenomenological character of time.”[242] He writes, “If the principle of general covariance is truly valid, […] the only time which a covariant theory can admit is an intrinsic time defined by the contents of the universe itself.”[243] He finally concludes that “when the whole universe is cast in the role of a clock, the concept of time can of course be made fantastically accurate (at least in principle) because of the enormity of the masses and quantum numbers involved. But as long as the universe is finite, a theoretical limit to the accuracy nevertheless remains.”[244] DeWitt argues that time is a convergent property of the universe, limited by the finite extent of the universe. He arrives at his intrinsic definition of time from the consequences of applying quantum constraints to the equations of general relativity, as well as from the general principle of covariance. This definition of time leads him to the conclusion that time is an approximate concept, due to

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[240] *Ibid.* 1137.

[241] *Ibid.*

[242] *Ibid.*

[243] *Ibid.*

[244] *Ibid.*

features of a universe described by the Wheeler-DeWitt equation, as well due to the finitude of the universe. The formalism of canonical quantum gravity, along with other considerations, suggests to DeWitt that, when considered in the context of the universe as a whole, time cannot be a precisely defined concept.[245]

In section ten, DeWitt explicitly engages in speculative and interpretive questions resulting from the formalism he has developed for canonical quantum gravity. He notes that much interpretive work has already been carried out in earlier portions of the paper, and that "the economy of quantum gravidynamics is […] revealed in the manner in which the formalism determines its own interpretation."[246] Here he is referring to the notion of intrinsic time he introduced earlier, as well as the way in which the seemingly static nature of the universe "forces us to abandon all use of externally imposed coordinates (in particular x^0) and to look instead for an internal description of the dynamics."[247] He then uses the relationship between formalism and interpretation to transition to a discussion of the work of Hugh Everett, whom DeWitt describes as believing that formalism and interpretation should be one and the same in quantum mechanics.[248] This is followed by a

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[245] As Sam Schweber has described, in the late 1930's researchers working on quantum field theory proposed a definition of a 'fundamental length,' a concept proposed by Heisenberg, in terms of the contents of the universe. However, such a definition became unnecessary with the development of renormalization theory. See Silvan S. Schweber, *QED and the Men who Made it: Dyson, Feynman, Schwinger, and Tomonaga* (Princeton: Princeton University Press, 1994), 593.

[246] DeWitt, "Quantum Theory of Gravity I: The Canonical Theory," 1140.

[247] *Ibid.*

[248] *Ibid*.

brief description of the Everett interpretation of quantum mechanics, and the idea that the "wave functions which undergo repeated fission, corresponding to the many possible outcomes of a given physical process," accurately represent reality.[249] DeWitt believes canonical quantum gravity naturally lends itself to the Everett interpretation, where the wave function is literally interpreted as the wave function of the universe. He writes that "[a]ccording to Everett, the wave function […] provides a faithful representation of reality; it is the universe itself which splits."[250] This is well suited to canonical quantum gravity, in which "one is accustomed to speak without embarrassment of the "wave function of the universe."[251] DeWitt takes this one step further, claiming "Everett's view is not only natural [to canonical quantum gravity] but essential."[252]

DeWitt then uses the Everett interpretation to explain the strange nature of time in the Friedmann universe; that is, the fact that wave packets don't "spread" in time, which led him to the conclusion that time is a "phenomenological" concept. After asserting that "the same must be true for the real universe," he draws the conclusion that the appearance of change in time in human experience is due to the multiplicity of Everett "branches", and that the universe itself repeats a single, monotonous "motion". He writes that the motion of the universe would be "repeated over and over again, like a movie film, throughout eternity, the monotony of which would be alleviated only by the infinite

[249] *Ibid*.

[250] *Ibid*.

[251] *Ibid.,* 1141.

[252] *Ibid.*

variety to be found among the multitude of simultaneous parallel worlds all executing the cycle together."[253] He thus sees the implications of canonical quantum gravity, with respect to time, to be deeply connected to interpretations of quantum mechanics.

Interestingly, this line of thinking leads DeWitt to a discussion of entropy in the final paragraphs of his paper. He writes that entropy's increase in time is a relative to a specific branch in the Everett interpretation. However, "for every Everett branch in which entropy increases with time there must be another in which entropy decreases with time. To an observer in the second branch "time" in fact appears to be "flowing" in the opposite sense."[254] DeWitt explains the apparent arrow of time as branch-dependent, with time-reversal invariance obtaining for the universe as a whole as described by the universal wave equation. This being said, he acknowledges recent experiments that suggest the violation of time-reversal invariance, likely the work of Cronin and Fitch on CP violation. He writes:

> it is difficult to say how these conclusions must be modified if, as recent experiments suggest, the real world is not invariant under time reversal. However, the world being as complicated as it is, it is still quite possible that there is no *preferred* direction in time. The ensemble of Everett branches in which time has given direction of flow may very well be balanced by another ensemble in which time flows oppositely, so that reality as a whole possesses no over-all time orientation despite the absence of time-reversal invariance.[255]

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[253] *Ibid.,*1142.

[254] *Ibid.*

[255] *Ibid.*

DeWitt contemplates the nature of time at several points throughout the paper. His treatment of quantum constraints and the Wheeler DeWitt equation leads him to an understanding of time as "intrinsic," and his later analysis of a wave-packet in the Friedman universe results in the conclusion that time is a "phenomenological" concept. Further, in the concluding section DeWitt directly connects these implications about time to the Everett interpretation of quantum mechanics, further developing his interpretive line of thinking about time in a quantum universe. Section 3.3 below will further explore the connections DeWitt draws between the Everett interpretation, quantum gravity, and time, bringing the nature of his interpretive engagement with time into sharper focus.

3.3 The Everett Interpretation

DeWitt's thinking about canonical quantum gravity was deeply connected to his views on interpreting quantum mechanics, particularly the Everett interpretation, as is clear from the concluding section of his 1967 paper. Hugh Everett had been a graduate student of Wheeler's at Princeton, completing his PhD thesis in March of 1957. The thesis was almost identical to a paper by Everett published in *Reviews of Modern Physics* in July 1957.[256] The edition of *Review of Modern Physics* in which the paper appeared was dedicated to a gravitation conference at Chapel Hill organized by the DeWitts, and Bryce DeWitt was one of the editors.[257] DeWitt later commented that when he first read the

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[256] Hugh Everett III, "'Relative State' Formulation of Quantum Mechanics," in *Review of Modern Physics*, 29 (July, 1957), 454 - 462.

[257] AIP interview of DeWitt by Ford.

paper, as part of the editorial team for the journal, he was "stunned" and "shocked" by the ideas.[258] In the paper Everett presented an interpretation of quantum mechanics that has since been controversial among physicists and philosophers alike, although it didn't became widely known among academic circles until the 1970s. DeWitt played a major role in gaining visibility for Everett's views by publishing an article in *Physics Today* in 1970, advocating for the interpretation. In this article DeWitt coined the term "many-worlds," which has come to be associated with Everett's ideas. DeWitt wrote the *Physics Today* article because he felt the physics community was neglecting Everett's ideas. He later commented: "[Everett] was being completely ignored. So I decided to write an article, a popular article, for Physics Today, which really put Everett on the map."[259]

The history of Everett's thesis and its reception is well documented in a paper published by Osnaghi, Feitas, and Freire in 2009. In this paper, the authors unpack early debates over the Everett interpretation that took place within Bohr's Copenhagen group in the late 1950s, as Wheeler tried to convince Bohr of the merit of his student's work. The paper includes extensive discussion of the context and genesis of Everett's thesis, as well as the content of the ideas. The authors summarize Everett's interpretation, using quotations from Everett's thesis and manuscripts, as follows:

> The ‘‘conceptual model of the universe’’ that Everett proposed ‘‘postulates only the existence of the universal wave function which obeys a linear wave equation.” […] Everett put it as follows: ‘‘The physical ‘reality’ is assumed to be the wave function of the whole

[258] *Ibid.*

[259] *Ibid.*

universe itself.''[260]

They go on to recount the debates that ensued following the publication of the thesis, particularly among Bohr's Copenhagen circle, revealing how deeply Bohr and his group adhered to the orthodoxy of the Copenhagen interpretation. When discussing DeWitt, they note that he was particularly amenable to Everett's ideas because he "had no sympathy for the Copenhagen interpretation."[261] They also note the connection DeWitt drew between Everett's ideas and his own work on quantum gravity, claiming that "DeWitt's interest in Everett's ideas was at least partly due to the role that they could play in the framework of his own research programme on quantum gravity."[262] DeWitt was an early champion of Everett's work, well before his ideas were discussed more widely within the physics community. He was excited by Everett's paper due to connections he drew to his own work on canonical quantum gravity, but also felt that interpretive question about fundamental concepts were of great importance for physics. The remainder of this section will consider DeWitt's views on Everett, as presented in his 1970 *Physics Today* article, to further develop a picture of DeWitt's interpretive engagement with concepts of time, quantum mechanics, and the universe.

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[260] Stefano Osnaghi, Fabio Freitas, and Olival Freire, "The origin of the Everettian Heresy," *Studies in History and Philosophy of Modern Physics,* **40** (2009): 107.

[261] *Ibid.,*120.

[262] *Ibid.*

DeWitt begins his 1970 *Physics Today* article by presenting his view on the measurement problem in quantum mechanics, followed by a description of three possible solutions, one of which is the Everett interpretation. In the article he refers to this interpretation as the "EWG metatheorem", describing it as an "assertion first given in 1957 by Hugh Everett with the encouragement of John Wheeler and has been subsequently elaborated by R. Neill Graham."[263] He uses the majority of the article to unpack the EWG metatheorem, which he believes to be the most promising of the three options. He writes:

> Of the three main proposals for solving this dilemma, I shall focus on one that pictures the universe as continually splitting into a multiplicity of mutually unobservable but equally real worlds, in each one of which a measurement does give a definite result. Although this proposal leads to a bizarre worldview, it may be the most satisfying answer yet advanced.[264]

DeWitt continues:

> The obstacle to taking such a lofty view of things, of course, is that it forces us to believe in the reality of all the simultaneous worlds represented in the superposition [...], in each of which the measurement has yielded a different outcome. […] The universe is constantly splitting into a stupendous number of branches, all resulting from the measurement-like interactions between its myriads of components.[265]

Recognizing this difficulty with the interpretation, he nevertheless believes that it is possible and likely, as Everett suggests, that the universe in reality splits. He directly

[263] DeWitt, "Quantum Mechanics and Reality, 33.

[264] *Ibid.*, 30.

[265] *Ibid.*, 33.

comments on this, demonstrating his willingness to engage in questions about the ontology of the universe and its relation to human experience.

It is clear throughout the article that DeWitt sees Everett's universal wave equation as akin to the Wheeler-DeWitt equation. He believes such an equation is unavoidable, writing: "If I am part of the universe, how does it happen that I am able, without running into inconsistencies, to include as much or as little as I like of the real world of cosmology in my state vector? Why should I be able, in practice, to avoid dealing with the state vector of the universe?"[266] DeWitt sees a line of connection between his work on canonical quantum gravity – and the Wheeler-DeWitt equation that is its centerpiece – and explicitly interpretive questions about the nature of the universe.

DeWitt makes it clear in this paper that he places a great deal of importance on questions of interpretation. He writes that despite the fact that the EWG interpretation does not yield experimental predictions, it has the "merit of bringing most of the fundamental issues of measurement theory clearly into the foreground, and hence of providing a useful framework for discussion."[267] He explicitly makes a claim for the importance of "philosophy of science":

> [T]he EWG interpretation of quantum mechanics has an important contribution to make to the philosophy of science. By showing that formalism alone is sufficient to generate interpretation, it has breathed new life into the old idea of a direct correspondence between formalism and reality. The reality implied here is admittedly bizarre. To anyone who is awestruck by the vastness of the presently known universe, the view from where Everett, Wheeler

[266] *Ibid.*

[267] *Ibid,* 35.

> and Graham sit is truly impressive. Yet it is a completely causal view, which even Einstein might have accepted. At any rate, it has a better claim than most to be the natural end product of the interpretation program begun by Heisenberg in 1925."[268]

DeWitt is explicitly engaged in what he sees as the "philosophical" side of physics, to which he attributes great importance. While he never strays far from technical discussions, he sees fundamental, ontological questions as central to his work as a physicist.

DeWitt saw a particular approach to interpreting in physics – that is, interpreting formalism literally - as embodied by Everett's ideas. He adopted this approach in his thinking about canonical quantum gravity and time, which helped him give meaning to the Wheeler-DeWitt equation, its implications for time, as well as shape his general ideas about interpretation in physics.

3.4 DeWitt, Physicist-Philosopher

Later in life, DeWitt distanced himself from the Wheeler-DeWitt equation and canonical quantum gravity. In a talk delivered in 1997, he explained: "Some of you here have heard me refer to [the Wheeler-DeWitt equation] as 'that damned equation.'" He attributed this to the fact that his "heart wasn't really in it," as even in 1967 he found the covariant path to quantum gravity much more promising than the canonical theory. He went on: "[the Wheeler-DeWitt equation] has played a useful role in getting physicists to frame

[268] *Ibid.*

important and fundamental questions, but otherwise I think it is a bad equation."[269] Even as DeWitt distanced himself from the equation, it is clear that he valued the fundamental questions it raised.

Interpretive questions about the meaning of fundamental concepts were highly valued by DeWitt, as is clear from his engagement with time in his 1967 paper, as well as his championing of Everett's work. Such questions were central to his canonical quantum gravity work, which was highly technical in tone. How did he come to this philosophical sensibility, uncommon during this period of American physics, in which he injected philosophical questions about fundamental questions into highly technical work? First, by selecting general relativity and quantum gravity as his primary research topic, and working exclusively on the problems involved, he began career on the fringes of mainstream physics. Yet his relationship with Wheeler, and Wheeler's work in promoting general relativity and quantum gravity research in general in the 1950s, helped DeWitt establish himself, and his style of doing physics, within the larger physics community. DeWitt's exposure to Everett's ideas further helped him develop his thinking about fundamental questions of interpretation.

Questions about time in the context of quantum gravity were also of great importance to Wheeler. In section four below I will look to Wheeler's early work involving the Wheeler-DeWitt equation, as well as his later views on time and quantum gravity, to further unpack the way this subgroup of physicists working on quantum

[269] DeWitt, "The Quantum and Gravity," 7.

gravity in the post-war United States questioned the meaning of fundamental concepts like time.

4. Wheeler

4.1 Superspace and Geometrodynamics

In the summer of 1967, Wheeler delivered a talk at the first Battelle Rencontres meeting in Seattle, organized by Cecille DeWitt and Wheeler himself. The proceedings of that meeting contain one of Wheeler's first recorded mentions of the "Einstein-Schodinger" equation. Throughout the published version of the lecture, Wheeler's tone is figurative and expansive. For example, in the opening paragraph he writes:

> one needs only to mount to this point of view to have the whole content of Einstein's theory spread out before his eyes, as in the outlook from a mountain peak. […] What is the nature of the landscape that we see from this height? What structures can we hope to build upon this landscape? And what kinds of mysteries are hidden in the mists beyond?[270]

Beginning on this lofty note, it is not surprising that Wheeler freely speculated about the nature of fundamental concepts, including the nature of space and time, in what followed. The "mysteries hidden in the mists beyond" are explicitly at stake for him as he discusses quantum gravity.

Geometrodynamics is a word Wheeler coined, based on the notion that, "the geometry of three-dimensional space is something that can undergo change as time

[270] Wheeler, "Superspace", 242.

passes, and propagate from one place to another, just as electromagnetic fields do."[271] Wheeler used the term "Superspace" to describe the background against which geometrodynamics takes place, writing: "geometrodynamics takes place in the arena of superspace."[272] In the introductory section of the lecture, Wheeler makes it explicit that the object he is interested in is three-dimensional space, changing against the backdrop of superspace, and not four-dimensional spacetime. He writes: "Here the dynamic object is not spacetime. It is space."[273] He does not find this surprising, noting that "in particle dynamics the dynamical object is not *x and t*, but only *x*."[274] He recognizes that this goes against the grain of current thinking in general relativity, and that it will be difficult for physicists to "change their minds and take back one dimension."[275] Nevertheless, he believes that "a decade and more of work" in the fields of general relativity and quantum gravity "has taught us through many a hard knock that Einstein's geometrodynamics deals with the dynamics of geometry: of 3-geometry, not 4-geometry."[276] While DeWitt is more nuanced on this point in his 1967 paper – singling out a 3-dimensional cross-section of spacetime, but ultimately avoiding external designation of space-like and time-like coordinates – Wheeler enthusiastically singles out space as distinct from time.

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[271] *Ibid.,* 268.

[272] *Ibild*, 242.

[273] *Ibid.,* 245.

[274] *Ibid.*

[275] *Ibid.*

[276] *Ibid.*

Wheeler's lecture begins with a discussion of classical geometrodynamics and the formulation of Einstein's equations by Peres. He refers to this as the "Einstein-Hamilton-Jacobi" equation, and notes some interesting features with respect to time.[277] That is, he believes that time, in the context of this classical equation, "means nothing more or less than *the location of the* ${}^{(3)}g$ *in the* ${}^{(4)}g$," with ${}^{(3)}g$ and ${}^{(4)}g$ standing for three-dimensional space and 4-dimensional spacetime, respectively. In this sense "3-geometry is a carrier of information about time."[278] Wheeler then comments on the meaning of time as he turns his discussion to quantum geometrodynamics, in which he believes the concept of 4-dimensional spacetime is no longer meaningful. Here he writes that, in quantum geometrodynamics, "the ${}^{(3)}g$'s that occur with significant probability amplitude do not fit and cannot be fitted into any single ${}^{(4)}g$. [One cannot] organize the ${}^{(3)}g$*'s* of significance into a definite relation to another." He concludes from this that the "time ordering of events is a notion devoid of all meaning."[279] He speculates further:

> Spacetime and time itself are not primary but secondary ideas in the structure of physical theory. These concepts are valid in the classical approximation. However, they have neither meaning nor application under circumstances when quantum-geometrodynamical effects become important. Then one has to forgo that view of nature in which every event, past, present, or future, occupies its preordained position in a grand catalog called "spacetime." There is no spacetime, there is no time, there is no before, there is no after. The question what happens "next" is without meaning.[280]

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[277] *Ibid.,* 251.

[278] *Ibid.,* 242.

[279] *Ibid.,* 253.

[280] *Ibid.,* 253.

Wheeler goes beyond DeWitt's claim that time is "intrinsic" or "phenomenological", directly asserting that "there is no time." He devotes several paragraphs to this speculative point about the nature of time, which he sees as one of the important conclusions to be drawn from quantum geometrodynamics.

Wheeler goes on to consider particular issues – the Planck length, gravitational collapse, and quantum fluctuations – in the context of quantum geometrodynamics. He then discusses the specific problem of the structure of superspace, at which point he introduces the "Einstein-Schrodinger equation." Of this equation he writes, "In geometrodynamics the structure of superspace is to be considered as defined entirely internally; that is to say, by the very form of the "Einstein Schrodinger equation" itself."[281] Unlike DeWitt, who first derives the Wheeler-DeWitt equation, and then draws interpretive conclusions from the equation concerning time, Wheeler begins his lecture with interpretive conclusions about time and space, which he then describes with the equation. While the two physicists differed in their paths and approaches, both Wheeler and DeWitt saw the "equation of the universe" in quantum gravity as deeply connected to interpretive questions about the nature of time. While Wheeler may have gone farther in drawing conclusions about the nature of time –and speculated more freely on the topic – both were engaged in questions about the meaning of fundamental concepts.

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[281] *Ibid.,* 278.

4.2 World as System

By the 1980s Wheeler's research interests had gradually shifted from general relativity to information theory, yet his interest in the meaning of the concept of time in the context of quantum gravity persisted. In his autobiographical reminiscences Wheeler described the shift in his interests in terms of his desire to understand the universe on the most fundamental level. He wrote:

> I think of my lifetime in physics as divided into three periods. In the first period, extending from the beginning of my career until the early 1950s, I was in the grip of the idea that Everything Is Particles. I was looking for ways to build all basic entities – neutrons, protons, mesons, and so on – out of the lightest most fundamental particles, electrons and photons. […] I call my second period Everything Is fields. From the time I fell in love with general relativity and gravitation in 1952 until late in my career, I pursued the vision of a world made of fields, in which the apparent particles are really manifestations of electric and magnetic fields, gravitational fields, and spacetime itself. […] Now I am in the grip of a new vision, that Everything Is Information.[282]

While in the grips of his "Everything is Information" period in 1988, Wheeler published an overtly philosophical paper in which he directly commented on the meaning of time. In this paper he discussed many ideas that he had first raised in the early 1970s, drawing philosophical conclusions that had come to be characteristic of his work. However, in this 1988 paper Wheeler places particular emphasis on the nature of time and its connection to earlier work in quantum gravity, which is why I have selected it for close attention. This following section will unpack Wheeler's ideas in this 1988 paper, looking to how

[282] Wheeler, *Geons* 62-63.

his engagement in philosophical thinking manifested itself in his later work, and the way in which questions about time and quantum gravity persisted in his thinking.

Wheeler's paper, entitled "World as system self-synthesized by quantum networking," is explicitly concerned with philosophical questions about "the secret of existence" and the "structure of the world".[283] The motivating idea behind the paper, which he first proposed in lecture in Oxford in 1974,[284] is that the "system of shared experience which we call the world [builds] itself out of […] elementary acts of observer-participancy."[285] Wheeler believes that the questions observers ask of the world, and the ways in which they communicate their findings, generate "that whole great system which to a superficial look is time and space, particles and fields."[286] The system, in turn, generates the observers. He illustrates this notion of a self-synthesizing system with the following illustration:

[283] Wheeler, "World as System," 4.

[284] C.M. Patton and J. Wheeler, "Is Physics Legislated by Cosmogony," in *Quantum gravity; Proceedings of the Oxford Symposium*, *England, February 15, 16, 1974* (Oxford: Clarendon Press, 1975), 538-605.

[285] Wheeler, "World as System," 4.

[286] *Ibid.*

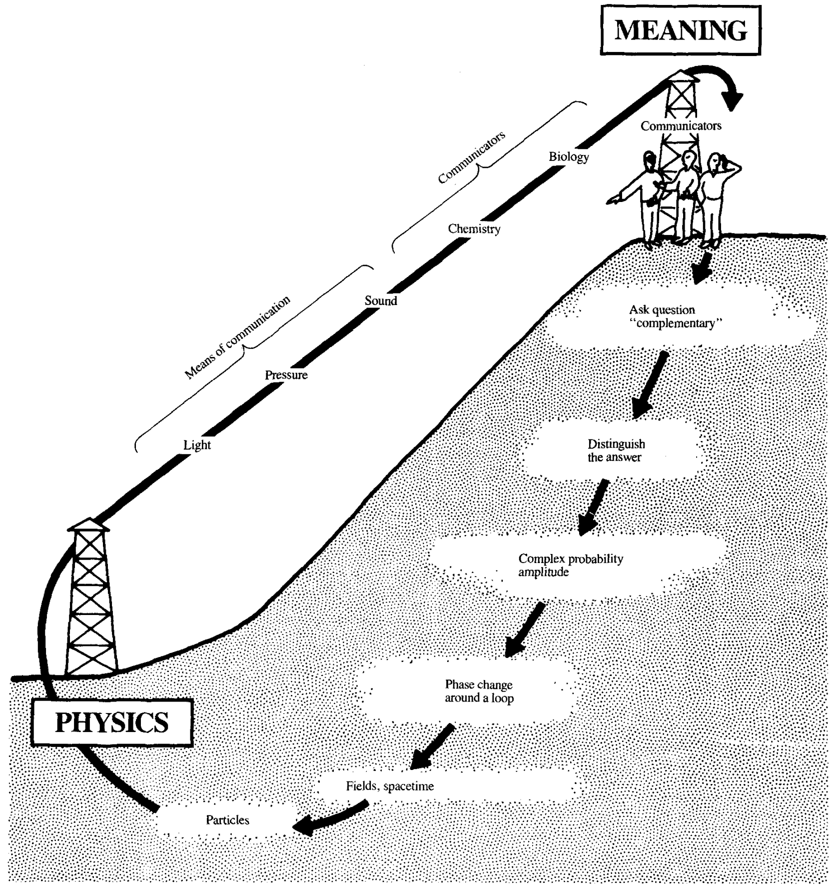

Figure 1

World viewed as a self-synthesizing system of existences. Physics gives light and sound and pressure–tools to query and to communicate. Physics also gives chemistry and biology and, through them, observer-participators. They, by way of the devices they employ, the questions they ask, and the registrations that they communicate, put into action quantum-mechanical probability amplitudes and thus develop all they know or ever can know about the world.

In a double-slit electron-interference experiment of the type proposed by Aharonov and Bohm, the interference fringes experience a phase shift proportional–so it is customary to say–to the flux of magnetic field through the domain bounded by the two electron paths. We reverse the language when we turn to the idea interpretation of nature. We speak of the magnetic field–and, by extension, spacetime and all other fields, and the whole world of particles built upon these fields–as having no function, no significance, no existence, except insofar as they affect wave phase, affect a 2-slit interference pattern, or, more concretely, affect the counting rate of elementary quantum phenomena. Fields and particles give physics and close the loop.

It is a strange business to report about what we don't know. It is no stranger, however, than recounting the first half of a detective story of which the second half is missing. We know how difficult it is to pick out the clues, let alone

Figure 3.1, from "World as System," p. 5

Wheeler structures his paper around four "clues" that "bear on the suspicion the quantum *is* the foundation of physics, that the world *is* a self-synthesizing system."[287] The clues are "no-continuum", "observer-participancy", "austerity", and "timelessness".[288] Regarding "timelessness" Wheeler writes:

> The deepest insights we have on time today come out of Einstein's 1915 and still standard theory of general relativity in its quantum version. This quantum geometrodynamics tells us that the very concepts of spacetime and of before and after break down at ultra-small distances. In tomorrow's deeper dispensation, we know that time cannot be an entity primordial and precisely supplied - as elasticity once seemed to be - free of charge from outside physics. Like elasticity, the very concept of time must be secondary, approximate, derived: derived from profound considerations of a quantum flavor.[289]

He concludes, "We must consider time as myth."[290] We see here that Wheeler's ideas about the nature of time stem from his understanding of quantum geometrodynamics. He brings implications about time, drawn from his earlier research program on quantum gravity, to bear on his new ideas about the universe.

He continues, with regard to timelessness:

> The concept of time was not handed down from heaven. Neither was it supplied free of charge from outside for the benefit of physics. The

[287] *Ibid.,* 6.

[288] *Ibid.*

[289] *Ibid.*

[290] *Ibid.,* 13.

> very word is a human invention, and the problems that come with it are of human origin. The miracle is only this, that a notion with so little undergirding has managed to stretch, without snapping, to encompass so much. Einstein's 1915 geometrodynamics continues to serve as the generally agreed authority for all that time now means and measures.[291]

Finally, he concludes: "Today time is in trouble. […] [T]here is no such thing as spacetime."[292] He understands this conclusion, which is similar to one he drew 20 years earlier with respect to geometrodyanics, as a clue to understanding "mysteries of existence."

Wheeler goes on to explicitly discuss the Wheeler-DeWitt equation, criticizing the whole quantum gravity project for presupposing that the universe exists in the first place:

> It is not enough in dealing with these difficulties to quantize Einstein's geometric theory of gravity according to the pattern for quantizing any other standard field theory; not enough to write down the resulting often-discussed wave equation […] not enough—despite all the fascination and instructiveness of the work of Everett, De Witt, Hartle, and Hawking towards interpreting the result—to calculate in this way the probability amplitude […] for this, that and the other 3-geometry. This whole line of analysis presupposes that there is such a thing as "the universe""[293]

For Wheeler, the "universe" does not exist, and neither does "time." Neither have meaning or existence outside of the self-synthesizing interaction between observers and a

[291] *Ibid.*

[292] *Ibid.,* 13.

[293] *Ibid.*

system.

Wheeler became increasingly comfortable and interested in making ontological claims regarding space, time, and the universe in the decades following his work involving the Wheeler-DeWitt equation in the late 1960s. Further, even when he moved on to pursue fields other than general relativity and quantum gravity, the work from that field in the 1960s – to which the Wheeler-DeWitt equation belongs – continued to influence the way that he thought about the nature of time and the universe.

4.3 Wheeler, physicist-philosopher

John Wheeler was well known for being a particularly expansive and speculative physicist. In an obituary in *Physics Today* published not long after Wheeler's death in 2008, Misner, Thorne, and Zurek put forward the view that Wheeler had "earned" the right to be speculative and bold due to the many successes he'd had in physics, and the overall respect he had garnered among the physics community in the process. As they wrote:

> Wheeler's many successes entitled him to examine crazy-sounding ideas without fear, one by one, aiming to discover which ones must be discarded and whether any of them should be taken seriously. [….] Wheeler's poetic imagination—with its deep, almost philosophical questions such as How come the quantum? and How come existence?—combined with his engineering common sense that brought many of his lofty ideas down to earth was his trademark way of doing physics.[294]

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[294] Charles Misner, Kip Thorne, Wojciech Zurek, "John Wheeler, Relativity, and Quantum Information," *Physics Today*, (April 2009), 46.

Along similar lines, in an obituary published in the *Proceedings Of The American Philosophical Society* Freeman Dyson described Wheeler as: " a conservative revolutionary, a prosaic poet, a calculating dreamer."[295] Dyson continues, describing the "poetic Wheeler,"

> Who asks outrageous questions and takes nothing for granted. The poetic Wheeler writes papers and books with titles such as "Beyond the Black Hole," *Frontiers of Time,* and *Law without Law.* His message is a call for radical revolution. "As surely as we now know how tangible water forms out of invisible vapor, so surely we shall some- day know how the universe comes into being. We will first understand how simple the universe is when we recognize how strange it is. The simplicity of that strangeness, Everest summit, so well directs the eye that the feet can afford to toil up and down many a wrong mountain valley, certain stage by stage to reach someday the goal. Of all strange features of the universe, none are stranger than these: time is transcended, laws are mutable, and observer-participancy matters."[296]

Wheeler came to exemplify the philosophically minded American physicist of the post-war period, who thought cosmologically about physics, engaged with fundamental ontological questions in his interpretations of physics, and drew speculative conclusion about the nature of time throughout his career. Questions about the meaning of time occupied him throughout his life. As he reminisced in his autobiographical notes: "Time is, in fact, an immensely complex idea that sits at the core of critical unanswered questions about the universe and existence, questions I can't stop pondering."[297]

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[295] Freeman Dyson, "John Archibald Wheeler," *Proceedings of the American Philosophical Society* **154** (2010), 126.

[296] *Ibid.,* 128-129.

[297] Wheeler, *Geons*, 351.

From his speculation about time and quantum gravity in the 1960s to his later discussions of observer participancy and timelessness in the 1970s and 80s, Wheeler was among the most visible physcists during the postwar period interested in fundamental and philosophical questions. Historians of science have attributed this characteristic to Wheeler's relationship with NIels Bohr. For example, David Kaiser wrote in *How the Hippies Saved Physics*, that Wheeler

> came of age in the 1920s and 1930s, a time when Americans who wanted to become theoretical physicists still had to travel to Europe to [...]. Wheeler studies with Niels Bohr in Copenhagen in the 1930s and often hosted his mentor during Bohr's many extended visits to Princeton after the war. These contacts hoped to stoke Wheeler's continuing philos[ohical engagement with quantum theory.[298]

Wheeler exemplified the American physicist who was technical, pragmatic, and speculative all at once.

5. Legacy in Quantum Gravity

In the years following its introduction in 1967, the Wheeler-DeWitt equation was viewed as a major achievement among the small group of physicists working in the field. Scholars such as Peres, Misner, Hawking, Kuchar, and Wheeler often cited the first installment of DeWitt's trilogy as the authority on canonical quantum.[299] Renewed

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[298] Kaiser, *How the Hippies Saved Physics*, 75.

[299] Asher Peres, "Canonical Quantization of Gravitational Field," in *Physical Review* 171 (July, 1968): 1335-1344; Charles W. Misner, "Quantum Cosmology .1." *in Physical review 186* (July, 1969), 1319-1327; Stephen Hawking, "Conservation of Matter in General Relativity," *Communications in Mathematical Physics* 18 (June, 1970): 301-306;

attention was given to the Wheeler-Dewitt equation in the late 1980s, when Ted Jacobson and Lee Smolin, working in the field of loop quantum gravity, found exact solutions to the equation, along with a more robust way of formulating it.[300] Loop quantum gravity is now one of the major alternatives to string theory, although it still represents a minority of researchers working on unification efforts, and a very small minority of physicists.[301]

Like Wheeler and DeWitt before them, researchers working on loop quantum gravity have used the equation, and the ideas about quantum gravity that accompany it, to ask deep questions about the nature of time. For example, in Lee Smolin's popular book *Three Roads to Quantum Gravity*, in which he describes his work on loop quantum gravity and the central role the Wheeler-DeWitt equation played in this work, Smolin directly discusses his understanding of the concept of time in physics. Early in the book he writes: "We now know that time also has no absolute meaning. There is no time apart from change. There is no such thing as a clock outside of the network of changing relationships."[302] Soon after, Smolin addresses the issue of whether "time exists" in the

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Karel Kuchar, "Canonical Quantization of Cylindrical Gravitational Waves," *Phys. Rev. D* 4 (August, 1971), 955-986; John A. Wheeler, "From Mendeleev's Atom to a Collapsing Star," in *Transaction of the New York Academy of Sciences* 33 (1971): 745-752.

[300] Ted Jacobson and Lee Smolin, "Nonperturbative quantum geometries," in *Nuclear Physics B*, 299 (1988): 295-354.

[301] For a discussion of string theory v. loop quantum gravity, see Dean Rickles, "Quantum Gravity: A Primer for Philosophers," in *The Ashgate Companion to Contemporary Philosophy of Physics*, ed. Dean Rickles, 2008; also see Lee Smolin, *The Trouble With Physics* (New York: Mariner Books, 2007), 239-258.

[302] Smolin, *Three Roads*, 24.

context of the universe as a whole, and particularly whether quantum gravity implies a "static universe" in which nothing happens. He writes, "It is absurd […] to speak of a universe in which nothing happens. Time is nothing but a measure of change – it has no other meaning."[303] He continues:

> One reason why it has taken so long to construct a quantum theory of gravity is that all previous quantum theories were background dependent. It proved rather challenging to construct a background independent quantum theory, in which the mathematical structure of the quantum theory made no mention of points, except when identified through networks of relationship. […] the theory that resulted is loop quantum gravity.[304]

Thus, according to Smolin's account of the history of quantum gravity, the question of how to understand time has been central to the field. He believes that the merit of loop quantum gravity lies with its interpretation of time. Further, he addresses questions that have historically occupied researchers in the field of quantum gravity – such the issues of the "static" universe – by leveraging his understand of what time "is". Ontological questions about time are central to his project, as well as the way he interprets the history of the field.

Similarly, physicist Julian Barbour, who has also worked on loop quantum gravity, has used the ontological interpretation of the concept of time as a lens through which to understand the history of the field. He explicitly believes the Wheeler-DeWitt equation implies that time doesn't exist, writing, "If one takes [the Wheeler-DeWitt

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303 *Ibid.*

304 *Ibid.*, 24-25.

equation] seriously and looks for its simplest interpretation, the picture of the universe that emerges is like the contents of the Timeless Theory bag."[305] He believes that this provides profound insight into the nature of time. He writes, "We can go on to ask what this tells us about time. The implications are as profound as they can be. Time does not exist. […] I see it as the only simple and plausible outcome of the epic struggle between the basic principles of quantum mechanics and general relativity."[306]

Questioning and interpreting the meaning of "time" has been central to loop quantum gravity, a research program that has taken up the Wheeler-DeWitt equation, and has been a central motivation for the physicists involved. Smolin, like DeWitt, saw quantum gravity as necessitating a new definition of time, while Barbour, like Wheeler, was comfortable with the bolder claim that "time does not exist." Regardless, the differences between these positions are largely attributable to semantics and style; all of these physicists believe quantum gravity provides a framework for ontologically complicating the concept of time.

6. Conclusion

The question of what time "means" has been of great importance in the history of attempts to unify quantum mechanics and gravity. Physicists have had to grapple with the fact that the fields of quantum mechanics and general relativity have historically conceptualized time differently, and that the concept of spacetime in general relativity

[305] Julian Barbour, *The End of Time* (Oxford: Oxford University Press, 1999).

[306] *Ibid.,* 247.

could not be uncomplicatedly translated into a quantum theory of gravity. Questions about the nature of time have therefore taken a primary role in unification efforts, including the canonical quantum gravity research program that has been the subject of this chapter. Different physicists within the canonical quantum gravity tradition have treated the issue in multiple ways. For example, DeWitt dealt with it by introducing his ideas of "intrinsic time" and "phenomenological time" and Wheeler addressed with it with bolder claims about the non-existence of time. Similarly, among a later generation of physicists, Smolin defined time in terms of the changing relationships within a system, and Barbour made bolder declarations that "time does not exist". Regardless of the subtle differences among these conclusions, it is clear the nature and ontology of time has been at stake in this work.

Among the physicists treated in this chapter, there has been a range in how bold or overtly speculative they have chosen to be in their discussions of time; nevertheless, all engaged in interpretive work, and saw insight into the nature of time as central to their projects. In this way, all of their understandings of what constitutes "physics" included philosophical interpretation. This is not to say that they had no sense of the boundaries between what they perceived as "science" and "non-science". For example, Wheeler wrote in his autobiographical reminiscences that he was upset when he learned that his work had been appropriated by what he termed "pseudoscientists."[307] He wrote:

[307] For a comprehensive account of the perceived distinction between science and pseudoscience among physicists during this period, see Kaiser, *How The Hippies Saved Physics*.

> To my discomfort and distress […] I found myself being increasingly cited by the pseudoscientists who were looking for scientific underpinnings for their moonshine. […] I didn't mind that some of my respected colleagues in science though that I myself had gone a little bit around the bend. They were entitled to remain more conservative, as I tried to be daring, But is did bother me greatly when I found my work cited as supporting the paranormal.[308]

This being said, the case of Wheeler, DeWitt, their quantum gravity equation, and its legacy shows that this small group of physicists included philosophical thinking within the boundaries of their discipline.

The type of interpretive thinking that characterized the subgroup involved with the Wheeler-DeWitt equation was exemplified by John Wheeler, who combined a pragmatic, typically American style of doing physics with an interest in philosophical questions inspired by his relationship with Niels Bohr. Further, Wheeler came to this mode of doing physics after having established himself in the physics community with his achievements in nuclear physics and his weapons work. Wheeler championed general relativity at a time when the physics community was generally hostile to the field, and helped carve out a space for other physicists to adopt his model of a philosophically interested, technically oriented physicist. DeWitt was part of this group, and brought a philosophical sensibility to his work on canonical quantum gravity, as well as his engagement with the Everett interpretation of quantum mechanics. This type of engagement with fundamental concepts, like time, carried on in the work of loop

[308] Wheeler, *Geons*, 343.

quantum gravity researchers who took up the Wheeler-DeWitt equation in the 1980s and 90s.

Unlike other more pragmatically minded physicists – such as those discussed in chapters one and two of this dissertation, who differentiated their work strongly from philosophy and more closely aligned themselves with engineering and applied physics – these physicists were more than willing to engage in speculative, interpretive questioning. They understood the question “what is time” to be central to the project of physics, while other physicists – both theorists and experimentalists –felt such an ontological question to be beyond the scope of their discipline. Physicists adopted a wide variety of attitudes toward fundamental concepts during this period, revealing a range of understandings of what it meant to be a postwar American physicist.

Further, this chapter reveals an additional dimension to the question, explored in chapters one and two of this dissertation, about how the concept of time in physics changed in the postwar United States, as well as the assumptions and presuppositions that have conditioned various physical concepts of time. Chapter one revealed contingency and heterogeneity at the level of timekeeping concepts and conventions, while also noting the presupposition of the consistency of time understood as an experimental variable. Chapter two showed how, in the context of postwar particle physics, time as a physical variable was itself at stake, while part of a multifaceted set of interfaces between theoretical and experimental practices, as well as a variety presuppositions about the role of time in physics. However, chapter two also revealed a basic presupposition about the consistency of time in theory and practice, and time’s existence was never called into question. Chapter three has looked to questions about the existence of time, calling into question presuppositions about the ontology of the concept. Nevertheless, basic understandings about time in physics, and the culture of postwar physics in which they still largely participated, constrained these claims.

Following the Wheeler-DeWitt equation reveals one slice of a heterogeneous picture of how physicists in the postwar United States understood the concept of time, as well as the role of interpretive questions in physics in general. For the group of physicists who developed and worked with the equation, philosophical questioning was central to the project of physics, and time's very existence was at stake.

CONCLUSION

1.

Each chapter of this dissertation considered an episode in the history of postwar American physics in which the concept of time was at stake. Chapter one looked to the physicists and astronomers involved in the development of atomic clocks and atomic time standards in the late 1940s and 1950s, whose work complicated physicists' definitions of basic timekeeping concepts. Chapter two looked to particle physicists in the early 1960s, whose worked challenged physicists' understandings of the direction of time in relation to physical laws. Chapter three looked to physicists working to unite general relativity and quantum mechanics in the late 1960s, whose work altered physicists' notions of time's ontological status in the context of the universe as a whole. In each of the three chapters time was cast in a variety of roles, for example as a unit of measure, a symmetry relation, or an element of spacetime. In addition, in each chapter different properties of time were at stake, including its universality, directionality, and ontology.

As the dissertation has shown, the subgroups of physicists discussed in all three chapters understood time differently; further each subgroup responded to the implications of their work for the concept of time in a variety of ways. When taken together, the chapters expose the diversity of understandings of and approaches to time among American physicists during the postwar decades, as well the variety of ways in which the concept of time was at stake during this period.

Two complementary lines of questioning run through the dissertation. The first asks about the professional identities of postwar American physicists in relation to their openness to philosophical investigation into the meaning of fundamental concepts like time. My argument here is premised on the idea that physicists' modes of engagement with time can offer insight into how they conceived of the boundaries of their discipline in relation to philosophical investigation. The second line of questioning concerns the role and status of the concept of time in postwar American physics. This line asks how subgroups of postwar American physicists understood time, how these understandings changed, and which assumptions and presuppositions physicists upheld along the way. In both lines of questioning I have used various elements of context as explanatory tools, including but not limited to institutional culture, instrumental and experimental techniques, intellectual traditions, disciplinary allegiances, and ideological values.

In this conclusion, I will set the findings of each chapter in relation to one another, in order to gain more general insight into the professional identities of postwar American physicists and their concepts of time. While doing so, I will draw on a set of philosophical tools – specifically from a line of thinking in continental philosophy - to help make sense of these insights, as well as extrapolate more general conclusions about the relation between postwar American physicists and their most fundamental concepts.

2.

2.1 Identity

The first line of argumentation, concerning physicists' professional identities, is set against the background of a tension between opposing sensibilities in twentieth century physics. The first sensibility, which many scholars have described as characteristic of European physicists in the first half of the twentieth century such as Albert Einstein and Niels Bohr, places investigation into the nature of fundamental concepts at the center of the practice of physics. Throughout the dissertation I have loosely referred to this as a "philosophical" approach to physics. The second sensibility, described by many scholars as prevalent among postwar American physicists, places a high priority on the production of useful results, to the exclusion of philosophical investigation. I have loosely referred to this as a "pragmatic" sensibility".[309] Each subgroup of physicists I have considered had a different relationship to this tension, as exemplified by how they approached questions about the nature of time raised in their work. At the most general level, the physicists considered in chapter one were firmly on the pragmatic side, the physicists in chapter two were in the middle of the spectrum, and those in chapter three were closest to the philosophical side. By looking more closely at each subgroup in question, the chapters

[309] As described in the introduction to this dissertation, I am using "philosophy" and "pragmatism" in loose senses to describe the general sensibilities I am after, while acknowledging that these terms themselves have complicated histories and relationships to the history of physics. For a discussion of the philosophical characterization of early 20th century European physics, as well as the pragmatic characterization of postwar American physics, see David Kaiser, *How the Hippies Saved Physics: Science, Counterculture, and the Quantum Revival* (New York: W.W. Norton and Company, 2011), xiii.

present a nuanced account of the orientation of each subgroup to this tension, offering insight into the way each subgroup understood the meaning and purpose of their work.

The physicists considered in chapter one were firmly situated within the pragmatic professional culture that largely characterized postwar American physics. They worked primarily in government institutions that emphasized practical applications of their work. Further, many of the techniques in which they were trained were pioneered by the prominent physicist I.I. Rabi, whose pragmatic vision for American physics – as a union of theory and experiment, generating good "citizen-scientists" – was transmitted to these physicists through direct lines of mentorship. Finally, the work of this subgroup was directly continuous with the frequency standardization research they conducted as part of wartime radar projects. The sense of practical purpose from radar was, to a degree, carried over to atomic timekeeping. Atomic clocks and standards complicated timekeeping concepts, blurring the boundaries by which they were circumscribed. However, the physicists involved in the development of these devices did not directly pose questions about these changes, due to their pragmatic understandings of their profession. Instead, they accused one another of failing to grasp basic principles, never grappling with the fact that their timekeeping concepts were in a state of flux. To engage in philosophical questioning of time would not have occurred to them as a professional possibility; conceptual questions about the meaning of concepts like time were not at stake for them.

The particle physicists considered in chapter two had a more complicated relationship to the tension between philosophical and pragmatic approaches to physics.

Fitch and Cronin identified insight into the nature of time as one of the implications of their famous 1964 experiment; further, they used this implication as one explanation for why the experiment was significant. In this way, insight into the nature of basic concepts, which was central to the philosophical sensibility, influenced how they conceived of and justified their work. Nevertheless, they never articulated concrete insights into the nature of time, nor did they engage in interpretive work about the concept of time in the context of their experiment. The philosophical sensibility was absent from their practice of physics, even though it was present at the level of their own explanations of the import of their work.

The physicists in chapter two displayed a hybrid sensibility toward physics, borrowing from both sides of the tension between philosophy and pragmatism. This hybrid sensibility can partially be explained by the position of postwar particle physics at the interfaces of experimental and theoretical practices emerging out of WWII physics, as well as newly forged relationships among physicists, machine culture, and instrumentation.[310] The work of this subgroup continued the project of wartime physics in many ways, working with many of the same machines, tools, and institutional structures developed during the wartime years. To an extent, this subgroup inherited a pragmatic sense of purpose alongside these machines, tools, and institutional structures. Yet, on the theoretical side, postwar particle physicists like Fitch and Cronin defined their project in terms of the search for the basic building blocks of nature. Their general mandate was to

[310] See *Image and Logic: A Material Culture of Microphyiscs* (Chicago: Chicago University Press, 1997).

uncover fundamental truths about nature, which placed insight into the concept of time well within their scope. Thus, particle physicists like Fitch and Cronin did not fit neatly within either side of the tension between philosophical and pragmatic sensibilities in physics; rather, their understandings of the meaning and purpose of physics were informed by both.

Finally, the physicists in chapter three, who were working on the unification of general relativity and quantum mechanics, approached physics with a largely philosophical sensibility. This group explicitly pointed out difficult conceptual issues concerning time arising from their work and directly engaged with these issues. In their published work and unpublished correspondence, they openly speculated about how to interpret basic concepts, like time, in the context of quantum gravity. While their discourse was constrained by their understandings of the boundaries of physics – for example, they drew clear lines between science and "pseudoscience" – interpretive questions about basic concepts fell within the practice of physics as they understood it. This more expansive view of physics can be partially attributed to the small size of the community of physicists working on general relativity in the 1950s and 60s, its often marginalization during this period, as well as the relation of many of its prominent members, such as John Wheeler, to the mainstream of physics. Further, the intellectual inheritance of this community - from the legacies of an older generation of physicists including Albert Einstein - made interpretive, philosophically inflected work easily palatable.

At a superficial level, this first line of questioning suggests physicists became increasingly open to philosophical questioning in physics as the postwar years unfolded. The physicists working in the late 1940s and 1950s did not see any place for philosophical questioning in their work, those working in the early 1960s were in the middle of the spectrum, and those working in the late 1960s were the most open to philosophical lines of thought. Yet each group's set of attitudes extended forward and backward from the episodes at stake in this dissertation. One conclusion to be drawn from this first line of questioning is that there were multiple identities available to postwar American physicists, each with contextually specific contours. There was no single way in which postwar American physicists circumscribed their discipline, and the boundaries they drew around their discipline were continually changing. This dissertation has described the particular ways in which several sets of professional boundaries took shape, by analyzing how different subgroups of physicists engaged with the concept of time.

2.2 Concepts of Time

The second line of investigation concerns the various ways time was conceptualized in physics during the postwar period, how concepts of time were challenged in this context, and what this can reveal about fundamental concepts in general in postwar physics. Chapter one described how the development of atomic clocks and atomic time standards placed the notions of a "clock," a "second," and "time" in flux. This was due in part to the fact that the mechanism of atomic timekeeping devices blurred the line between a clock, a frequency standard, and a time standard. Atomic timekeeping devices involved systems of resonance, feedback, and calibration that were novel to timekeeping; the conceptual boundaries of clocks and seconds within such systems were not yet worked out. The changes in timekeeping concepts during the late 1940s and 1950s were influenced by several contingencies, belonging to the agendas of the specific scientists involved. For example, William Markowitz's disciplinary allegiance to astronomy affected the way the atomic second eventually took form. Further, Harold Lyons' values surrounding accuracy and universality in timekeeping affected the way he approached building an atomic clock. The changes that took place surrounding understandings of timekeeping concepts - such as a "clock", "second", and "time" - in the wake of the development of atomic clocks, were bound up with the specific agendas and values of the scientists involved.

Chapter two described how Fitch and Cronin's experiment and analysis called ideas about the direction of time, and the role it plays in the laws of physics, into question. The experiment showed that in certain instances, the laws of physics yield different predictions in the forward and backward directions in time, challenging the consensus view of the nature of time as physical variable. Fitch and Cronin arrived at their result within the context of particle physics, at the intersection of accelerator experimentation and particle theory. It involved a highly technical concept of time reversal invariance in physics, along with several understandings of time carried over from different subcultures of physics. While the concept of time as a physical variable was challenged by Fitch and Cronin's experiment, it did not result in concrete changes in the way time was used and understood by the particle physics community.

In chapter three, physicists working on the unification of quantum theory and relativity asserted that the ontological status of the concept of time needed to be reevaluated. For example, due to the absence of the variable "t" in an equation they derived as a universal equation of quantum gravity, they suggested that time does not exist. Further, in this context they pointed out several other reasons why the application of quantum principles to general relativity rendered the concept of time poorly defined or arbitrary. Their understandings of time were bound up with a specific notion spacetime originating from general relativity, which explicitly cast time as an object of investigation. Within this tradition, the concept of time was overtly at stake, and physicists called its existence into question.

This second line of questioning reveals a variety of changing concepts of time among postwar American physicists. Further, these concepts were all grounded in a range of assumptions, each influenced by specific institutional, cultural, and technical contexts. Yet were there any assumptions or presuppositions common to the multiplicity of concepts of time at stake during this period? In chapter one I noted that despite the multiplicity of timekeeping concepts involved, physicists assumed that the concept of time as it appears in the equations of physics – the variable "t" - was consistent and commensurable among theories and practices. Nevertheless, a close look at understandings and uses of variable "t" in chapter two revealed a multiplicity of further concepts, assumptions, and presuppositions. Thus, that which was presupposed as unified in the context of chapter one was revealed to be deeply heterogeneous in the context of chapter two. However, the consistency of "t" was taken for granted on certain levels in the context of chapter two as well, even though it was a composite of many concepts and assumptions, different from those at work in chapter one. At the level of basic presuppositions, the physicists in chapters one and two took the consistency of "t" for granted, even if these "t"'s were different among the different subgroups. Further, on an even more basic level, the physicists in chapters one and two presupposed the "existence" of time. Chapter three's physicists directly challenged the assumption that time exists. Yet were there any unexamined presuppositions about time taken for granted by the physicists in chapter three? Even if they questioned whether time existed, within a specific and idiosyncratic notion of "existence", they presupposed that, in discussing time, they were speaking about a meaningful concept. Thus, all three groups shared in the basic presupposition about the meaningfulness of the concept of time.

This second line of questioning reveals a heterogeneous picture of the concept of time in postwar American physics. This picture involves a range of assumptions and presuppositions at multiple levels, as well a shared, unexamined presupposition about the meaningfulness of the concept of time. This shared presupposition did not contain concrete content about the nature of time; rather, it asserted the conceptual existence and coherence of time as such. This presupposition about the meaningfulness of time was built into the ways these physicists formed and used the concept, although it did not attribute any well-defined qualities to the concept of time itself.

Can the heterogeneity the concept of time, and the assumptions and presuppositions that have conditioned this heterogeneity, including the presupposition of time's meaningfulness as such, be used to develop an understanding of how the concept of time has historically gained meaning? What, if any, is the relationship between the presuppositions that have conditioned the understandings and uses of time in postwar American physics, and the concept of time itself? This type of question – about the relationship between a concept and the presuppositions that condition it – has many elements in common with a variety of twentieth century intellectual traditions. One such tradition is a line of continental philosophy that builds upon the work Martin Heidegger, which has influenced the ideas presented in this dissertation. At this point I would like to analyze the value of the structures offered by this Heideggerian line of thinking for understanding the physical concept of time, as understood by the physicists discussed in this dissertation. I will not be looking to how Heideggerian thinkers have treated the philosophical concept of time itself; rather, I will consider the way this tradition has understood concepts in general, along with the role of presuppositions in conceptualization, to provide further nuance and contour to my own thinking about the nature of time as a fundamental concept in postwar American physics.

3.

I would like to consider a specific philosophical structure, concerning a form of "negativity" that runs through Heidegger's thinking. On a basic level, this Heideggerian negativity is the opposite, or absence, of "positive presence"; at the same time, for

Heidegger it is an essential component all that is positively present. Heidegger describes this negativity in the introduction of his influential *Being and Time*.[311] Here, he identifies his methodology as "phenomenology," going on to describe a phenomenon as "something that lies *hidden* […]; but at the same time […] something that belongs to what shows itself, and it belongs to it so essentially as to constitute its meaning and its ground."[312] Phenomenology, for Heidegger, is directed toward the determination of that which lies *hidden* in what shows itself. Ultimately, Heidegger's phenomenological project in *Being and Time* seeks to reveal that which lies hidden *as* hidden: concealment as such. Even in Heidegger's later works, in which he does not explicitly describe his method as "phenomenology", the structure whereby the conditions of possibility of that which is present can only be concretely understood as *hiddenness*, or concealment, remains.

In every phase of his work, Heidegger draws attention to that which is hidden in that which is positively present.[313] He describes such hiddenness as the condition of possibility for all positively present concepts or entities; further, he claims that all positive modes of thinking are incapable of registering the negativity of this hiddenness.

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[311] Martin Heidegger, *Being and Time*, trans. John Macquarrie and Edward Robinson (New York: Harper, 1962).
[312] *Ibid., 59*.
[313] Heidegger's career is often divided into "early", "middle", and "late" stages. See for example Hubert Dreyfus and Mark Wrathall, "Martin Heidegger: An Introduction to his Thought, Work, and Life" in *A Companion to Heidegger, Volume 20 of Blackwell Companions to Philosophy*, eds. Hubert Dreyfus and Mark Wrathall (Malden MA: Blackwell Publishing, 2007), 1-15. While there are profound differences in Heidegger's objectives and emphases throughout these phases, I argue that the negative structure I describe has played an important role in all three stages.

In other words, all positive modes of thinking are structurally incapable of registering the negative presuppositions on which they are founded. Accordingly, he believes the task of philosophy is to orient itself toward this negativity, concretely registering it, in order to overcome the constraints of positive modes of thought. According to Heidegger, positive modes of thought have dominated the Western philosophical tradition; further, they have formed the basis of natural science. Thus, he believes modern science will never be able to register its negative conditions of possibility, for to do so from within science would necessarily presuppose the positive presence of these conditions. In other words, the negative conditions of possibility of science will never be available to scientific modes of thinking. Philosophy, for Heidegger, has the potential to orient itself toward concealment as such. Orienting oneself to this concealment would be, for Heidegger, the proper way to approach fundamental concepts taken for granted by science, including the physical concept of time.

For example, in a 1954 lecture "Science and Reflection," Heidegger claimed that "[i]f the sciences themselves should at any time be able to find at hand within themselves what is not to be gotten around of which we are speaking, they would have before all else to be in a position to conceive and represent their own essence. But they are never in a position to do this."[314] Heidegger does not believe the sciences are capable of conceiving of their own "essence". For Heidegger, the alternative to positive scientific inquiry would not involve making the truth of science and reality positively present, but would rather

[314] Martin Heidegger, "Science and Reflection," in *The Question Concerning Technology and Other Essays* (New York: Harper and Row, 1977), 176.

consist in a proper orientation to its concealment. This would overcome the difference between science and its negative conditions of possibility. Heidegger does not see science as flawed or false, but rather as characterized by the lack of resources to properly conceive of its conditions of possibility, which inherently defy conception.

At first glance, this negative structure suggests two things with respect to the ideas about time and physics presented in this dissertation. First, it suggests that physicists' relationships to fundamental concepts like time cannot be completely conceived of within the framework of physics itself. While, as this dissertation has shown, there have been many ways in which physicists have engaged with the concept of time, and just as many ways they have understood the meaning and purpose of physics, one could argue along Heideggerian lines that in all cases physicists have been committed to the project of positive conceptualization. And while they have analyzed, used, and speculated about many aspects of the concept time, they have never directly dealt with its conditions of possibility, which make meaningful discussion of the concept possible on any level. In all cases the presupposition *that* time is meaningful precedes all investigation into the concept of time, and thus cannot be the object of such investigation. Second, this Heideggerian structure suggests a possible method by which a philosophically minded history of science could approach the concept of time in physics; that is, with attention to its negative, inherently hidden conditions of possibility. While an understanding of the nature of time would not be possible within modes of philosophy that take positivity for granted, one taking a Heideggerian approach could orient itself to the negative foundations of the concept of time, and in this way gain some access to that which defies conceptualization in terms of positive presence. Yet could such a Heideggerian analysis of time in physics ever be possible? Would it be able to access that which is *a priori* excluded from the concept of time *per se*, or would it continually commit the error of trying to force time within the constraints of positive conceptualization?

Other philosophers who have followed in Heidegger's tradition have taken up the question of whether there are any positive possibilities for philosophy in relation to the negative foundations of concepts, and the difference between a concept and its conditions of possibility. For example, French philosopher Jacques Derrida has claimed that philosophy will never overcome the difference between a concept and the negativity, which is *a priori* excluded from the concept, in which it is grounded. Unlike Heidegger, Derrida does not see the task of philosophy as the proper orientation to concealment as such, in order to ultimately overcome the difference between a concept and its conditions of possibility. Rather, philosophy will always and necessarily repeat this difference in the act of pointing to it. Thus, all to which philosophy could or should aspire is the continued repetition of the difference between a concept and its conditions of possibility. Consider the following quotation from Derrida's *Aporias*, which is devoted to a reading of Heidegger's *Being and Time*:

> The work exceeds itself, it surpasses the limits of the concept of itself that it claims to have properly while presenting itself. […] When someone suggests to you a solution for escaping [this] impasse, you can

> be almost sure that he is ceasing to understand, assuming that he had understood anything up to that point.[315]

For Derrida it would be impossible to properly register the negative conditions of possibility of a concept and fulfill the philosophical agenda laid out by Heidegger. Thus, if one accepts Derrida's position, then an account of time in physics could only ever go as far as pointing to the elusive nature of the concept, while repeating the error of taking its positive presence for granted. This would suggest that just as the physicists considered in the dissertation presupposed the meaningfulness of the concept of time, so has this dissertation itself. While pointing to the heterogeneity and contingency of time, and unpacking the assumption that there is a truth to time, I have also presupposed such a truth. For Derrida, this is inescapable; the inherent contradiction involved in this type of analysis is unavoidable.

This being said, other thinkers who have engaged with Heidegger's thinking, such as Italian philosopher Giorgio Agamben, have argued that philosophy must aspire to more than the repetition of the difference and contradiction inherent to conceptualization; philosophy could and should attempt to achieve something positive. Agamben discusses the negative structure raised in Heidegger's work primarily with respect to the nature of language. He focuses on the presupposition that language exists, or what he refers to as the "event of language," which structurally precedes all meaningful discourse. Along Heideggerian lines, he argues that this event/presupposition is the condition of possibility

[315] Jacques Derrida, *Aporias* (Stanford: Stanford University Press, 1993), 32.

of all language, but is entirely and necessarily absent from that which is said. In *Language and Death* he writes:

> The scission of language into two irreducible planes permeates all of Western thought […] The very structure of transcendence, which constitutes the decisive character of philosophical reflection on being, is grounded in this scission. Only because the event of language always already transcends what is said in this event, can something like a transcendence in the ontological sense be demonstrated.[316]

Agamben believes this structure is the essence of metaphysics, and that language is necessarily and exemplarily metaphysical. Nevertheless, he believes the task of philosophy is to overcome metaphysics and achieve something positive, a task he believes Heidegger proposed but was unable to achieve.

Agamben makes several suggestions over the course of his work for what a positive achievement in philosophy would entail.[317] While the specific details of such positive suggestions are beyond the scope of this dissertation, it raises the question of whether such a positive philosophical conclusion is even possible, and what it would look like. Is it enough to point to the heterogeneity of time at on a variety of registers, and the contentless presupposition of the meaningfulness of time, in order to understand the nature of time in terms of the negativity in which it, and conceptualization in general, is grounded? Or should one attempt to say something positive about the nature of time, and

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[316] Giorgio Agamben, Language and death, *Language and Death: The Place of Negativity* (Minneapolis: University of Minnesota Press, 1991), 82.

[317] Positive solutions to metaphysical problems take different forms in different instantiations of Agamben's work, from poetry, to ethics, to religious notions of messianism. For an overview of Agamben's work, see Leland de la Durantaye, *Giorgio Agamben: A Critical Introduction* (Stanford: Stanford University Press, 2009).

overcome the negative foundations of the concept? Would such a positive statement even be possible, and if so what would it look like? I tend to agree with Derrida's position, acknowledging that all one can do is continually point to the absence of a positive essence of time, and that one necessarily mischaracterizes this absence while doing so. By looking at the concept of time in a variety of contexts - noting its heterogeneity, and tracing the presuppositions that condition it - I hope to have pointed to the absence of an essence of time, as well as used this absence to help understand the concept of time that I have necessarily presupposed as positive along the way.

Time is a topic Heidegger wrote about extensively, and Heidegger's account of time was deeply bound up with his central ideas about human existence and death.[318] However, this dissertation has focused on the concept of time *in physics*, placing time as a category in Heidegger's thought far outside the scope of the concepts at stake in this project. Yet Heidegger's position on the negative conditions of possibility for conceptualization, as well as the responses of Derrida and Agamben to this aspect of Heidegger's work, can help in a final analysis of the concept of time in postwar physics. This being said, Heidegger, Derrida, and Agamben all conceive of concepts as detached from historical particularities. While the negative structures they discuss can provide tools for approaching the concept of time in physics, and conceptualization in general in science, their methods can only go so far toward accessing the historical specificities of concepts in the particular contexts in which they emerge. Thus, the ideas of these continental philosophers must be taken together with those of historians and philosophers

[318] For example, Heidegger's accounts of time and temporality are central to division II of *Being and Time.* See Martin Heidegger, *Being and Time*.

of science, who grapple with physics as a deeply situated enterprise, to gain a deeper sense of the meaning of time, and fundamental concepts in general, in physics.

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This dissertation has revealed the concept of time in postwar American physics as deeply heterogeneous on a variety of registers. Postwar American physicists understood time as a unit of measure, a physical variable, a background for change, and part of the fabric of spacetime, to name a few of the many roles in which time was cast. Further, they assumed that time possesses a variety of properties, including, but not limited to: universality, contingency, symmetry, existence, and meaning. Building on continental philosophical thinking, one can add to this list the heterogeneity of the concept of time in relation to its conditions of possibility, a heterogeneity that comes with any concept conceived of within a positive, scientific tradition.

In addition to deepening the heterogeneous picture of time, consideration of the structures presented by continental thinkers such as Heidegger, Derrida, and Agamben can help one orient oneself to this heterogeneity - and the absence of a positively present concept of time, or set of well-defined concepts of time - within physics. Moving beyond the fact that there are many different concepts of time, and assumptions about time, within physics, this tradition can help frame the question: what can be learned from this heterogeneous picture of time in a particular historical context? By situating time within the context of subgroups of postwar American physicists, this dissertation has highlighted the specificity of different concepts of time on the many registers in which time functions within physics. This specificity speaks to the more general insight that there is no single, unified concept of time to be found within physics. Further, by tracing the contours of this specificity we can see the ways in which the absence of a unified concept of time gives meaning to the concept as understood within different episodes in physics, as the condition of possibility for any positive concept. Thus, a historically specific, differentiated account of the concept of time in physics – taken together with more general ideas about conceptualization in modern thought, and the role of philosophical attention to historical specificity – can help provide insight into the fundamental absence that underpins the concept of time in physics.

4.

In this dissertation I have situated physicists in the postwar United States in relation to the competing pulls of philosophy of pragmatism, in terms of their engagement with the concept of time. Further, I have examined the different ways these physicists understood time, the ways these understandings changed, and the various levels of assumptions and presuppositions at work. Finally, in this conclusion I have considered concept of time in physics on a philosophical register – borrowing from a line of thinking from continental philosophy – to explore what can be said about the concept of time, and concepts in general, in physics.

Overall, the dissertation has detailed the movement of shifting professional boundaries of physics, along with some contours of the deeply heterogeneous, and ultimately elusive, concept of time. Time in physics, and the presuppositions on which it is founded, continue to change; alongside these changes, new ideas about what it means to be a physicist, and the nature and essence of the concept of time itself, continue to be stake.

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